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EYE-MOVEMENT DURING VISUAL IMAGERY

BY
EDITH TOTTEN
The Johns Hopkins University

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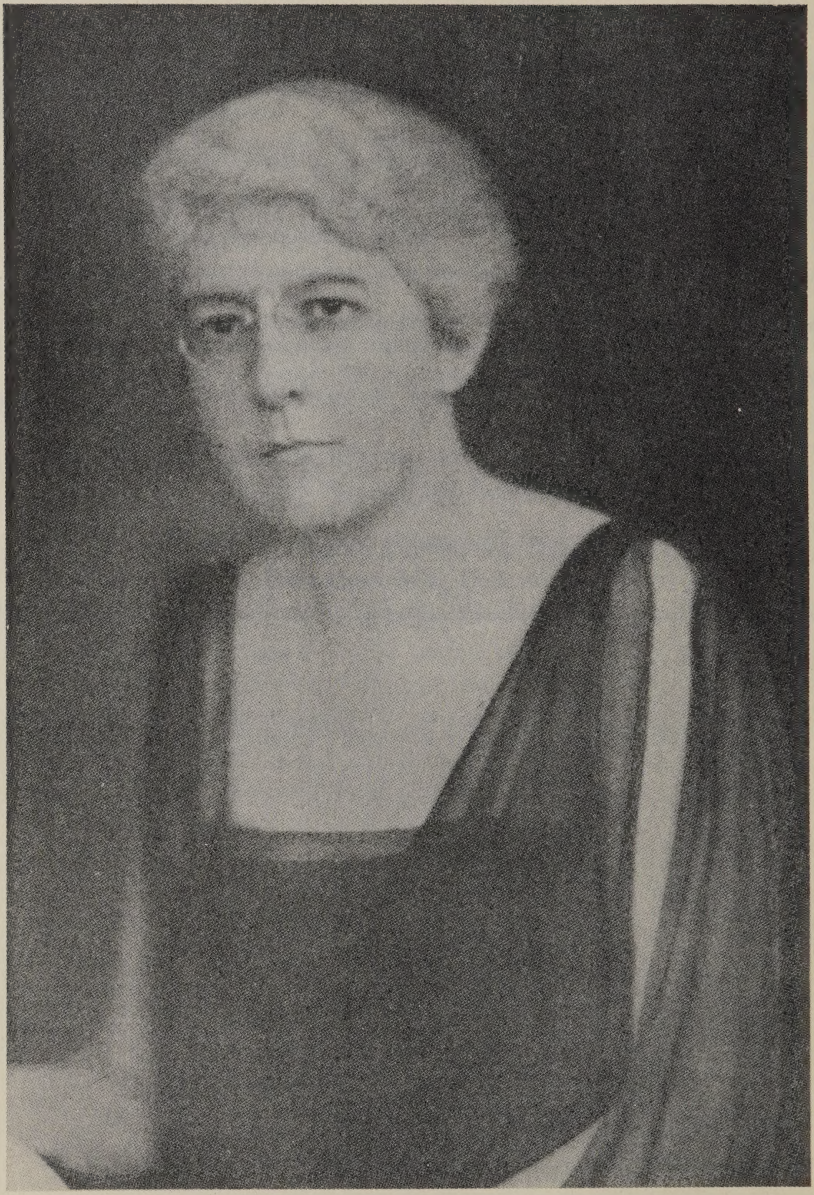

Edith Totten

EYE-MOVEMENT DURING VISUAL IMAGERY

EDITH TOTTEN

From the Psychology Laboratory of the Johns Hopkins University

FOREWORD

The work reported in this monograph was completed by Dr. Totten, and the report written in 1928. Shortly after completing the draft, Dr. Totten presented an account of her work before the Psychology Seminary, deeply impressing those present with both presentation and contents. She seemed in excellent spirits, evidencing the satisfaction natural to the accomplishment of a laborious undertaking. On finishing the presentation, and as discussion was commencing, Dr. Totten fell forward in apparent unconsciousness, and in spite of the efforts of a physician present, and another quickly summoned, did not recover. Death was presumably due to paralysis of the respiratory mechanism. Thus was lost a psychologist of high character and admirable personality, who had demonstrated great originality, ability to persist in tedious experimental routine, abundant critical ability in the devising of problems and control of methods, and extraordinary facility with apparatus. The loss was the more severe, because she had completed arrangements to devote the remainder of her life entirely to research.

Dr. Totten's manuscript has been copied with the least possible editing, and that in no significant points. She would have revised it before printing, if she had lived. The development of the problem and methods was primarily her own: the credit given me in her statement being overly generous. Publication as a memorial to Dr. Totten has been made possible through the generosity of her classmates of the Bryn Mawr class of 1902.

KNIGHT DUNLAP.

I. THEORETICAL

The specific theory in regard to the rôle of eye-movement in visual imagery, which the following piece of research may help to illuminate, is based upon the general theory of imagination or thinking, the details of which, together with his theory of the development of perception, has been from time to time since 1914, developed and formulated by Dunlap (2, 3, 4). The general theory may be stated as follows: According to the reaction hypothesis, no activity of the organism is conscious unless it involves a complete reaction. For the purposes of the present discussion, it is assumed that the reaction must be completed by a contraction of striped muscle. Thus, in the perception of a tree, for instance, first the receptors of the retina are stimulated, then follows a series of nervous changes by which the impulse, started in the retina, passes to the visual centers of the cortex. The process is not yet complete, however, and the organism has no conscious experience of "tree" until the nervous impulse has passed on by way of efferent neurons to a muscle or group of muscles and has given rise to a muscular contraction. It is further a part of the theory that in the course of individual experience, contraction of certain definite groups of muscles or muscle fibres becomes habitually the terminal response to a specific receptorial stimulus pattern. That is, in repeated perception of "tree A," the same group of muscle fibres is always involved in the terminal muscle pattern of the reaction. The *total* terminal muscle pattern will not necessarily be repeated each time, since "tree A" will not always appear the same, but portions of the pattern must be repeated in order that the tree may be recognized as A and not B.

In thinking or imaging, in contrast with perceiving, the pattern of muscular contraction which constituted the terminal part of the response in the original perceptual experience, recurs without a recurrence of the receptorial stimulation which determined the *initial* response in that experience.[1] Thus in imaging "tree A," the retinal receptors are not involved, but the image is experienced when the terminal muscle pattern of some previous perception of "tree A" recurs, variations of pattern and in degree of tension corresponding with variations in color, in form and in position in space.

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[1] An exception to the statement that the receptors starting the reaction in thinking are not the same as those which started the reaction in perception, must be pointed out in the case of "thinking of" a muscular movement as perceived through kinesthesis. Here the thought is probably conditioned by a repetition, to some extent, of the original contraction.

In making a detailed application of this theory to visual imagery, we may readily point to a set of muscles which are habitually activated in response to a certain sort of retinal stimulation and which therefore suggest themselves as the agents of the terminal response in the corresponding type of visual experience. These muscles are the extrinsic muscles of the eye, and the perceptual experience which their contraction would appear to help to condition, is the experience of spatial relation in the case of objects whose position in space, or the relations of whose parts demands a movement of the eye for clear vision. When an object, or part of an object, is out of the line of direct vision the rays of light fall upon peripheral parts of the retina, and if a clear view of the object is wanted, adjustments of the extrinsic muscles of the eye instantly occur in bringing the fovea into position to be stimulated. Here we have a complete chain of events from receptorial stimulation to terminal muscular contraction, upon which the individual's experience of spatial relation in part depends. I say "in part," because pupil and lens are also concerned in focussing upon the kind of objects in question, but their changes involve still another group of muscles.

If it is true that the extrinsic muscles of the eye do furnish the terminal responses conditioning the conscious experience of visual spatial relation, in the circumstances I have specified, then we should expect, in the light of our theory, that when an object is imaged as if in these circumstances, the spatial relations appropriate to it would be added to the image through a recurrence of the contractions of the eye-muscles and movements of the eyes which have previously conditioned their perception. In "imaging" an object extending in vertical position in space, for example, there would occur a series of eye-movements vertically placed. The muscular changes would be less in extent, no doubt, than in perception, but of the same character.

II. THE PROBLEM

This investigation is concerned with possible changes of tension in the extrinsic muscles of the eye, in visual imagery, as indicated by movements of the eye-ball. It falls into two parts, each of which attempts to answer one of the following questions: (1) Do eye-movements such as would be required in the perception of an object, occur in "imaging" that object? (2) Is "visual imagery" possible without such eye-movements?

The experiments covered in this paper are concerned with the first question only. Experiments dealing with the second question have already been begun and will be described later.

For much of the original scheme of these experiments, I am indebted to suggestions from Dr. Dunlap.

III. HISTORICAL

In reviewing the work bearing upon the question of eye-movement in visual imagery, it seems hardly possible not to begin with Stricker, although his little book, *Stüdien über Bewegungsvorstellungen,* published in 1882, does not profess to be an exposition of conventional laboratory experiments. It is, nevertheless, one of the most interesting utterances we have on this subject. Stricker had made frequent and careful observations on his own "images of movement" and discovered that it was impossible for him to imagine objects in motion, without experiencing in one part or another of his own body, very lively "sensations" of muscular movement. In imagining himself or other human beings as walking, these "sensations" occurred in the muscles with which he would make similar movements. By making a special effort and attending especially to his eyes, however, the muscles of his eyes could be made to serve in place of those of the legs (p. 16). In imagining a man as floating in the air it was always the eye muscles that were brought into play, and sensations from these muscles usually furnished the movement element in recollections of small animals or inanimate objects in motion (p. 17). Even in imagining the color yellow changing to blue the actual *change* of color could not be imagined without the help of eye or neck muscles (p. 18). After watching a motion picture of a man jumping, arranged by exhibiting in a toy stroboscope a series of pictures each one of which represented a man in one of the phases of jumping, Stricker reports, "If immediately after the observation I allow the memory image to appear, I notice that my eye-balls actually change their positions with the idea 'up' 'down.' But I can suppress this change of position without destroying the idea of movement. I bring about this suppression of movement by pressing the lids against the eyes. But now instead of real eye-movements, lively feelings of innervation appear. I innervate the muscles as if they were following the jumping man, and only in this way can I obtain the memory image of motion" (p. 31). Stricker distinguishes clearly, however, between "images" of movement for which muscular "sensations" were essential and "sensory" images in which he believed the peripheral nerves (as he says) played no part (p. 37).

Since Stricker, students of visual imagery have very generally kept in mind the matter of eye-movements, noting passing references to them in the introspective reports of their observers, or, in a few instances making the search for them a part or the whole of their problem. The studies of Slaughter, 1902, (12), Meakin, 1903, (7), Moore, 1903, (8), and Murray, 1906, (9), are introspective studies purely. Eye-movements are reported to a greater or less extent in all of them, although in none of them is the question of eye-movement an integral part of the problem.

In 1910, Perky (10) published a series of experiments in which she undertook to determine whether "images of memory" and "images of imagination" could be differentiated on the basis of the occurrence of eye-movement in one type and its absence in the other. The introspection obtained in a preliminary set of experiments had led her to believe that eye-movements were to be found in "memory images" but not in those of imagination. A device was introduced into the technique by which eye-movements would be brought strikingly to the subject's attention. The procedure is described as follows: "The observer sat in a dark room, his head supported in a head-rest, and his left eye screened. The right eye fixated a luminous (phosphorescent) spot 1.5 m. distant. Four other and similar luminous spots, at the same distance from the eye, were placed within and near the upper, lower, right and left margins of the blind spot of the fixating eye. A sixth luminous spot was placed to the right just beyond the limit of the field of vision. So long as fixation was continued, or the fixating eye moved only within certain narrow boundaries, the outlying lights remained invisible; so soon as movement of the eye attained a certain extent, some or all of the outlying lights flashed into the field." "Careful and detailed introspections were taken of the course and character of the image, and a note was made of the appearance of outlying light or non-appearance of the outlying lights while the image was in course." "The observers were left altogether in ignorance of the object of the work; they soon discovered that the movements of the eye were in question, but no one, of them, so far as we are aware, realized that we were in search of distinction between image of memory and image of imagination" (p. 437). The stimuli used were words, or later, sentences, the experimenter deciding from the nature of the descriptions of the images whether they were to be classed as images of memory or of imagination.

There were six observers. "Of the 212 memory images, 191 (or 90 per cent) involved eye movements to the extent that outlying lights came into the field of vision; of the 214 images of imagination, 146 (or 68 per cent) showed no evidence of movement. Of the 68 images of imagination for which movements were recorded, 12 were images of animals conceived as running swiftly across the field of vision, and 17 were images of greater extension than could be covered by the resting eye, images of a stretch of landscape, or of the side of a near or large building. If we rule out these as equivocal, we have only 21 anomalous cases of memory (no movement) and 39 anomalous cases of imagination (movement recorded)" (p. 438). "In the next experiment the experimenter selected the stimulus words with a view to avoidance of very extended images, and images of moving animals." This change in technique gave 96 per cent of cases involving eye-movement in memory images, and 74 per cent of cases without eye-movement in images of imagination. "But in spite of precautions, 30 of the 43 refractory cases of imagination were cases in which the observer imaged an extended scene. Of the 122 cases without eye-movement, 6 were also of extended scenes, but here no estimate could be made (as was ordinarily the case) of the distance of the scene from the observer” (p. 438).

The method used here is ingenious and interesting, although, admittedly, it was not expected that the finer eye-movements could be detected by it. There is the further objection, also, that looking out for the lights would tend to cause eye-movements which bore no relation to the images and also to interrupt the imagery. All these objections, however, would in all probability affect images of all types equally. Assuming that the observers in this set of experiments were fairly accurate in their reports, and the experimenter unbiased in her classification of the reports, it is clear that for some reason in this group of six people there were manifested eye-movements of greater magnitude during the imagery classified as “memory” than in that classified as “imagination.” The basis of classification was as follows: “There were,” says the author, “on the one hand, images of recognized and particular things, figuring in a particular context, on a particular occasion, and with definite personal reference; and there were on the other hand, images with no determination of context, occasion or personal reference,—images of things recognized, to be sure, but not recognized as this or that particular object. The former were evidently ‘images of memory’; the latter, both by positive and by negative character, were ‘images of imagination’ ” (p. 436). This in itself suggests a reason why there was more eye-movement reported with the former than with the latter group of images. Suppose the stimulus word were *man.* If I “image” *man* as a Roman senator, it would require an effort to see him in this realistic way; my image would appear more as the reproduction of a photograph. If eye-movements occur in “imagery” under circumstances such as would require them in perception, as the theory of the preceding section maintains, Perky’s “images of memory” would be such as would be especially likely to yield them, not because of the memorial character of the “images,” but because their size would require movement of the eye in order to represent the spatial relations of the parts.

An examination of the stimulus words and the introspections might help to a decision as to the reasonableness of this suggestion, but these are not given. There is nothing to show that any systematic analysis of the results was made, with such an idea as this in mind, although the discrimination and proper evaluation of "images" representing objects of "greater extension than could be covered by the resting eye," might have suggested doing so.

Martin's study, 1912, (6) included a small amount of work on the question of eye-movement. She was interested in obtaining further evidence on the conclusion reached by Perky that eye-movements occur generally in "memory images" but only rarely in "images of the imagination." Her subjects were required to call up simultaneously two "images," one a "memory image" and one an "image of imagination," to hold these and compare them from a number of different points of view. Among other things they were to report the occurrence of eye-movements and movements of the body with either type of image. She states her results as follows: "Along with both images movements were 'felt' by the three observers; but they were less pronounced with G. All occasionally saw movements especially with memory images. G. sometimes 'saw' movements in the image, where others 'felt' them. Eye-movements occurred in connection with movements as they were reported in seeing the whole image, in the working out and examination of the details, in turning towards the more pleasing or interesting image, with laughter over an amusing feature, etc. Apparently such movements were somewhat more strongly defined with memory images, except for the cases in which memory images occurred in the construction of fantasy images which did not arise spontaneously."

Ogden, 1913, (11) performed some experiments with a variation of Perky's method. Words were presented to the subject, one by one, on exposure cards. Above the printed word on each card, a disk was pasted which the subject was required to fixate while getting an "image" suggested by the word. Distraction marks in black ink were placed "near the left side of the aperture" through which the stimulus words were exposed, the idea being that if eye-movements occurred during "imagery," these marks would be seen. In only four cases was it reported that the marks were seen simultaneously with the production of the "image."

In Clark's study, 1916 (1) a preliminary series of experiments furnished the basis for a three-fold classification of the "images" that occurred in response to a list of 114 words,—images of specific and familiar objects (F-images), images of specific but unfamiliar objects (U-images), images signifying a class or a member of a class (G-images). Three series of experiments were then carried out in which the attempt was made to correlate the presence of eye-movements with (1) F-, U-, or G-"images"; (2) shifts in attention; changes in clearness; associated processes; somatic reference; visual background of image; specificity of object and spatial and temporal relations; familiarity; (3) presence or absence of meaning. Three different methods were used to detect the occurrence of eye-movements. In the first series a high light on the cornea was obtained from an electric bulb and the experimenter noted movements of the eye by observing this point of light through a hand lens. "The point of light fell upon the edge of the pupil. When the eye shifted, the margin of the pupil moved noticeably nearer to or farther away from the dot of light, which remained approximately stationary." Although it may be permissible to say that the bright spot remains "approximately stationary," depending upon what is meant by "approximately" the bright spot is by no means stationary, as such studies as those of Stratton (13) had already abundantly shown. The movements, therefore, which could have been detected by this method would have been only a small selection from those actually occurring. Of 96 images studied by this method, only 33 showed eye-movement. Of these, 32 occurred in connection with the specific-familiar and the specific-unfamiliar images. Only one was noted in connection with the "general images." These "F- and U-images" were characterised by usually having scenic background, while the "G-images" were without; "F- and U-images" had wealth of detail, while "G-images" had little; all of the images of exaggeratedly large or small size were among the

"U-images"; somatic reference was characteristic of the "F-images." If the eye-movements occurring with visual "images" are merely recurrences of such as normally occur in the perception of objects of the character of those visualized, we should expect greater amplitude of movement with images of large size and with background, (since background extends the limits of the total image), and a larger number of movements when details are noted. So it would seem that the results here show, (if they show anything in regard to the "images"), a correspondence between eye-movement and those characteristics of the "image" which would require eye-movements in order to secure clear vision in perception.

In the second series of experiments, an objective record of movement was obtained by using the "writing tambour of the laryngeal recorder described by Kreuger and Wirth, which was connected by rubber tubing to the nystagmograph of A. Schackwitz." The observer was seated with his eyes closed and the tambour, adjusted to the headrest, was placed against the right eye-lid. Movements of the eye were thus to be recorded on the smoked paper of a kymograph. There was no way of distinguishing between movements of the eye-lid or head, and those of the eye-ball. Moreover, the experimenter determined by a final set of experiments, in which the tambour was used on one closed eye, and Perky's lights were used for the other open eye, that many movements were demonstrated by the appearance of the lights which made no record on the kymograph. Thus, clearly, only gross movements of the eye-ball, besides, undoubtedly, head-movements and movements of the lid, were detected by this method. Provision was made for the subject to record fluctuations in the intensity of the images. The author claims that in 128 cases of movement out of 172 "a correspondence appears between the records for fluctuations of the clearness of the 'image' and the records for ocular movements." In other words the movements were reflections, not of images but of the interruptions to, and difficulties in producing imagery.

In Clark's third series of experiments Perky's method was used. Outlying lights were so arranged as to fall on the blind spot during fixation, and on the sensitive portions of the retina when the eye shifted enough to permit fixation of a point 5 mm. distant from the fixation point on a screen 1.5 m. from the eye. In these experiments the objects to be visualized were both meaningless and meaningful drawings, apparently not more that 2 by 2.5 inches in size, exposed in order. Later, the observer was to visualize these drawings as they were named to him. It turned out that the meaningless drawings had meaning for the observers except in 7 out of 88 cases, and there was no appreciable correspondence between "images" of this sort and the presence or absence of eye-movements. The experimenter found that "fixation was usually steady when an 'image' was very clear, when it was accompanied by few associated processes, and when attention was without effort. Under other conditions ocular movement tended to occur." However, it is important to note that of the three observers, C, D, and F, C reported movement in all images, F in all but one, while D reported no movement in 23 and movement in but seven. Although movement occurred in all of C's "images," 20 were very clear and 19 fluctuated. Although no movement occurred in 23 of D's 30 "images," yet the very clear and steady ones constituted only "over half" the number. Clearly, the correlation between movement and lack of clearness is not very close. The absence of movement in D's images must be traceable to D's individual way of reacting to the particular sort of stimuli that were used.

In Clark's conclusions of the three sets of experiments, the author says, "Eye-movements seem to us to be symptomatic of those general central conditions which underlie *determination* and which fix the state and configuration of consciousness, i.e., fix attention and associative completion. . . . Visual perceptions normally flow in trains, passing from feature to feature of the object and likewise from object to object. . . . We believe that whenever this exploratory and diffusive attention occurs, there normally appear . . . the small movements of the eyes which have now been detected in many experiments set for the study of imagery. . . . The presence and frequency of ocular movements would have no immediate or intrinsic connection either with 'familiarity,' or with 'personal reference' and 'particularity'. . . . We are thus lead to connect ocular movements more directly with general central and conscious conditions than with special functional differences displayed by the image." The author seems to hover on the brink of relating eye-movements in visual imagery to the definitely directed movements required for the perception of objects in space, and it is disappointing to find her diverted to the notion of eye-movements as indications of "diffusive and exploratory attention," "general conditions of consciousness" (pp. 490–491).

IV. APPARATUS AND TECHNIQUE

In 1902 Stratton published his first piece of work on eye-movement (13), and described his use of an enlarging camera for photographing the movements of a point of light reflected from the surface of the cornea. No one so far has used this technique for a study of eye-movements in visual imagery. It offered a very promising method for such a study and was chosen for the experiments that I wish to describe.

The enlarging camera (the same instrument that Stratton used), as I used it, was extended to about four feet and gave an enlargement of about four times. Owing to the difficulties of focussing at the same time, both the corneal reflection and the bead used as a control on head movements (to be described later), it was not possible always to use exactly this extension of the camera.

As Judd and his collaborators (5) pointed out, in using the corneal reflection to record movements of the eye, a certain amount of the movement is lost, since the light spot does not move exactly as does the eye. If the cornea were a section of a true sphere the light spot would not move at all, but owing to the deviation of its shape from the spherical, the movement of the eye does carry the spot, and the photographic record of it is practically useful in such work as this. To what extent the enlargement obtained with the camera I used counteracted this loss of movement, I cannot say, but undoubtedly its effect must be assumed to be irregular, in some instances, that counterbalancing the loss, in others exaggerating the changes in position.

Because of the inequality between the movements of the bright spot and those of the eye; and also because the fixed plate photographs give no information as to the temporal relations of the fixations of the eye; I hoped to be able to use, for part of the work, a white spot fixed to the surface of the cornea, photographing it with a motion picture camera. A satisfactory white spot was devised (16), but the motion picture camera proved to be inadequate for the purpose since the focus obtainable with it does not give a clear enough outline of the spot to make it possible to trace minute changes in position of the white spot. Photographs of the white spot on a fixed plate were also unsatisfactory, so the corneal reflection and enlarging camera were used throughout the experiments.

The camera was fitted with an excellent rapid rectilinear lens and was used with the full aperture of the lens.

In order to secure a corneal reflection that would be as nearly as possible a "point of light," an Edison-Swann "Pointolite" lamp of 100 candle power was used. The corneal reflection obtained with this lamp, with careful focussing, gives a very neat little photograph of a brief fixation of the eye. A perfectly exact focus of the tiny spot of light was, however, not attainable with the R. R. lens fully open. But on the other hand, restricting the aperture would have diminished the amount of light admitted to too great an extent. All the light possible was to be desired in order not to diminish the chances of recording fixations of briefest duration.

In regulating the focus of the corneal reflection, a very clearly discriminable series of changes in the form of the image on the ground glass could be observed—the result of the slight inexactness of the focus of the lens made fairly conspicuous by the enlargement. With too short a focus the image on the ground glass appeared as a tiny horizontal slit of light. As the focus was lengthened the slit widened a bit in the center and a vertical cross piece appeared. With continued lengthening of the focus, the horizontal line disappeared, the vertical line developed into a lozenge of light and finally into a vague blur. The change in length of focus sufficient to produce a single step in this series was not more than a millimeter or two. It was never possible to find a focus at which the image on the ground glass appeared to my eye as a mere point of light. The photographs were always taken after the cross shape had been located on the ground glass, with the subject's gaze directed straight ahead. Even after the most careful focussing, however, the photographs did not always present the cross shape. Very small changes in the direction of the subject's glance was sufficient to alter the focus, and result in a vertical lozenge or a horizontal dash in place of the cross.

Investigators in the field of eye-movement have recognized the necessity not only of employing means of holding the subject's head steady, as far as it is possible to do this, but also of using some device by which may be detected movements, which may occur in spite of all precautions. These measures were especially important here, because, in the first place, the subject had to be relieved, as far as possible, from the necessity of attending to anything but his task of imagery, and in the second place, because of the possibility not only of random movements of the head, but also of movements which might occur vicariously for eye-movements (since it is a fact that objects are frequently brought into direct vision by a movement of the head instead of a movement of the eye).

There were two supports for the head—a thin block of wood, held by an iron support and placed between the subject's teeth, and a head rest which was strapped around the sides and over the top of the head.

As a control to record head movement I used a steel ball, about 1.5 mm. in diameter, mounted on one end of a strip of aluminum covered in black, the other end of which was bent around the temple piece of a pair of celluloid spectacles. This celluloid bar was mounted at one end on a block of cork, which was bound to the subject's head, just above and in front of the right ear. In this way the steel ball was held out at the side of the right ear. Its position was regulated, for the purposes of focussing, by sliding the strip of aluminum along the celluloid bar.

When the enlarged prints made from the negatives came to hand, I was rather startled by the unexpected variety of size and form shown in the photographs of the head control, many of which, at first glance, were suggestive of head movement. Yet there were certain features of some of the photographs which made me feel that they could not indicate head movement. In order to elucidate the matter, if possible, I took eight photographs of the head control fastened to a rigid support. Four of these photographs are shown in figure 52, plate VI. The two upper ones show the steel ball, in different positions in relation to the camera and light (but both of them, positions which it probably occupied at some time or other during the regular experiments), focussed as carefully and perfectly as I could get it. The image of the ball on the ground glass never presented the cross shape of the first photograph, to my eye, and deciding just when the best focus had been obtained was difficult, as the range within which the spot of light seems clear and bright is appreciably greater than is the case with the corneal reflection, although this range is, in itself, still very small. The two lower photographs were taken with the ball in the same position in relation to the camera, as the cross-shaped one, but with the focus, as I judged, a little out, though accurate enough so that there was no evidence of blurring. For the left hand lower image the focus was shortened, as compared with the more perfect focus; for the right hand one it was lengthened. It is clear to me that all but a few of the photographs of the head control which aroused my suspicion, are explained by these four photographs. Each of the two lower forms is merely one or the other of the cross bars of the upper left hand form, the other cross bar having been eliminated by the change of focus. Two other photographs taken with the ball in a slightly different position in relation to the camera, and with similar changes of focus, show similar oblique forms, sloping in opposite directions to one another. The clue for the interpretation of the photographs of the head control, came of course from the series of changes observed in connection with the corneal reflection. The photographs of the head control in its position of the head, are somewhat more blurred than in figure 52, owing to the fact that the ball on the head was subjected to a certain amount of jarring from the heart beat and respiration, capable of affecting the plate under the conditions of enlargement of these photographs.

The light from the "Pointolite" lamp is very trying to the eyes. In order to minimize the subject's distress from this cause, the lamp was enclosed in a wooden box, furnished with a window, 4 by 5 inches in size, in which was set a piece of rather thick blue glass (in imitation of Dodge's procedure). Thus the light was softened without too much reducing the actinic rays effective for photographic purposes. The light in its box was placed on a table about 6 feet from the subject, to his left and towards the front, so that it gave a good reflection on the cornea, without intercepting the line of vision. Both eyes were open, but only the right was photographed. The room was darkened, except for the illumination from the "Pointolite" lamp. This was not sufficient to give a photograph, usually, of anything but the brilliant high light on the cornea and the reflection on the head control. Occasionally there was an indication of some high light on an out-lying portion of the eye-ball, but there was never the smallest possibility of confusing this sort of thing with the images from the other two sources.

The subject was seated in front of the camera, turned a little away from it, so that he might look towards or at a black curtain hung ten feet in front of him. The camera was placed a little to the right of the right eye and as close to it as was possible without allowing the frame of the instrument to obstruct the line of vision. Its position brought the lens about 7 cm. from the eye.

The photographic plates used were the "Sigma" Lumière Dry plates, 4 by 5. For each experiment, two plates were used, placed about two inches apart in the plate-holder. This was necessary because, with the enlargement employed, the images of the corneal light and the steel ball were too far separated to fall upon any single plate that could be conveniently used.

The objects chosen for visualization were such as would demand, in perception, groups of eye-movements significantly different from one another in spatial arrangement. Thus, it was believed that the groups of eye-movements that might be recorded during visualization could be related in a general way to the object visualized. I say "in a general way," because Stratton showed that the eye, in ordinary observation of an object, does not follow the contours of the object and that therefore the pattern of eye-movements is not identical in shape with the form of the object under examination. But clearly, the group of eye-movements brought into play in observing a very high vertical object should be significantly different from the group employed in examining a long horizontal object. Any differences should be apparent between these groups and those necessary for a cross-shaped object. We might therefore expect to find similar differences in the groupings of eye-movements that might occur in imaging objects whose parts are spatially related in these ways.

The objects given to all subjects for visualization were:

1. The Washington Monument in Baltimore, which is a cylindrical shaft, set upon a square base, with a square platform at the top, on which stands a small dome bearing a heroic figure of Washington. The height of the monument is, I believe, 225 feet.
2. A freight train standing still on the track.
3. Two men sitting, one at each end of a long table. This was not given to two of the subjects.
4. A large cross. In some cases it was to be a "flaming cross," in others it was a roughly made wooden cross which was exhibited to the subject. The wooden cross measured seven feet in each direction from tip to tip. At its four extremities I had placed four designs in order to draw the attention out to the full extent of the cross shape.

Other objects used in one or more cases were:

1. A colored print which was exhibited to the subject, and in which the objects represented were placed both vertically and horizontally, but with the greater extent in the vertical direction.
2. A symphony orchestra in action, forming in general, a horizontally arranged configuration.
3. A train, an engine, an automobile in motion.
4. The Washington Monument seen as if slanting at an angle of 45 degrees.

5. The Washington Monument seen as if in horizontal position.
6. The Washington Monument seen large and nearby, and the same seen small, these constituting pairs of experiments with the same conditions of focussing and with the same subjects.

Some rather small diagrams drawn on paper were used in a few experiments, but were found to be too small to be useful, and were discarded.

Early in the work I realized that the character of the results would be entirely different according to whether the objects were visualized as if seen from nearby and large, or as if from far off and small. (This point I found later suggested in Perky's work.) Since we should expect, in visual imagery, only such eye-movements as would be used in bringing an object and its parts into clear vision in perception, we should not expect to get eye-movements if the object were visualized as so small that it could be seen entire at a single glance. Also, little if any movement would be expected if the object were imaged as if the subject were fixating one point, seeing that clearly and getting the outlying portions in peripheral vision. Unless these matters were cared for, the results were pretty sure to be irregular. I therefore worded the instructions with this in mind. The form of instruction was as follows: "When I say 'go', please get a visual image of the Washington Monument seen as if from nearby, so that it will appear large. Be sure to see all the parts, base, shaft, and top." Directions for the train and cross were similarly phrased, and those for the two men at table were made explicit by requiring the subject to visualize the men as if they were at either end of a long table and he at the side of the same table.

My custom was to tell the subject what the task would be before giving the formal instructions. This was to avoid the chance of confusion and hurry on his part at the moment of taking the photograph. New tasks were always practised before the experiment. After the formal instructions had been given and the signal, "Ready, Go," the cap was removed from the camera. In the early experiments I timed the exposures myself, replacing the cap at the end of from ten to fifteen seconds, depending upon the novelty or apparent difficulty of the task. My reason for doing this was to avoid placing any responsibility upon the subject, beyond that of securing the visual image. This procedure, however, was unsatisfactory, since occasionally it interrupted the image, and frequently gave so much time that the imagery was repeated. In later experiments, therefore, the subject was instructed to close his eyes when he had completed the image, and I stopped the watch and replaced the cap over the lens at the closing of the eyes. Since the amount of illumination did not permit the photographing of the eye-lid, the closing of the eye merely put an end to the experiment.

As soon as the lens was covered, the plate holder closed, and the light turned on, the subject was released and his account of his visualizing experience written down.

V. THE SUBJECTS

The subjects were seven undergraduate students of the University and two Doctors of Philosophy in Psychology, a man and a woman, both trained in the introspectionalist method. Of the two introspectionalists, *B.*, the woman, was entirely conversant with the purpose of the experiments, in all its details. I have never doubted that *K.* understood as well what was afoot, but I purposely did not discuss it with him during the progress of the experiments, and he left town before I had an opportunity to talk the matter over with him at their termination. Of the students, *Hof.* was the only one who had had enough psychology to have met with the theory of eye-movements in connection with visual imagery. On talking with him at the end of the period of experimentation I found that he had guessed correctly the purpose of the experiments at the outset. He was certain that this knowledge had not influenced his reactions, and that if eye-movements did occur it was as an inevitable accompaniment of the visualizing experience. A comparison of his results with those of the other subjects will be the best test to which this belief can be subjected. *Bas.* and *Rit.* had had one course in elementary experimental psychology, but I lost them both prematurely, without having a chance to find out what they knew or guessed.

The other four, *Shil.*, *War.*, *Re.* and *Hi.* had had no training in psychology. *Hi.* guessed correctly that I was looking for eye-movements in "picturing things in length and breadth," similar to those occurring in "really looking at" things, but he was not at any time conscious of eye-movements. *Re.* and *Shil.* thought I wanted to find out if the "image" in imaging was inverted as the retinal image is in vision. *War.'s* idea was that I was somehow trying to photograph some sort of image in his eye.

If I am asked how I knew that my untrained subjects were actually having the experience which we call "visual imagery," I must answer that I based my judgment on the character of the descriptions which they gave me of their experiences in response to the requirement that they "get a mental picture" of the Washington Monument or that they "see it in the mind's eye," in preliminary talk with them, as well as on the character of their reports during experiments. Examples of these reports will be given later in connection with the plates and can be compared with those of the two trained subjects.

It turned out that the two introspectionalists, *B.* and *K.*, were somewhat handicapped by certain features of their training, in performing exactly the task I had for them. Both had done a good deal of work in visual imagery and both had been in the habit of visualizing *during* instructions. They found that the images came as usual during the giving of the instructions, and then it was difficult and unnatural for them to begin again at the signal "go," and get the image afresh and in accordance with the completed instructions. This difficulty could, no doubt, have been surmounted more completely than was the case if they could have worked with me long enough to build up the other habit. *K.* was also somewhat troubled at the outset by the general conditions of the experiments, feeling hurried and not quite at ease. This disappeared before the end of the experiments.

I was interested in finding out, as a side issue, whether any of my subjects represented in their imagery the so-called "edidetic" type (i.e. whether or not they projected their images), and whether the photographic records would show any distinctive differences corresponding to the projected and non-projected images. It turned out that one subject, *Rit.*, always projected his images, that *K.* and *B.* and *Bas.* sometimes did so, and that *Hof.* could do so when he chose. *Hof.* had not before known that his images could be projected but found after a little practise, that the projected image was easier for him than the non-projected. His naïve surprise when he first succeeded in projecting an image, (which was much facilitated by a mild illumination of the black curtain), was the best proof I had of the genuineness of his experience. I discussed the matter with him nevertheless, at some length in order to be satisfied that he really did project. He differentiated between the two sorts of experience by saying that in the non-projected image he saw background, while in the projected image he saw only the single object by itself, and that the projected image was like a "movie" thrown on the screen. It will appear from the plates, I think, that whatever the fundamental difference may be between these two sorts of image, it is not a matter of presence or absence of eye-movement.

VI. THE EXPERIMENTS

Before considering the experiments it may be well to look for a moment at some photographs, which give a notion of what the method can do with voluntary eye-movements of known character, and which also present examples of certain facts in regard to eye-movements and the movements of the corneal reflection, which Stratton has already pointed out. Figures 53 and 54 in plate VI, show the records of eye-movements involved in the examination of a vertical line formed by a series of 5 rectangular papers fastened to a plumb line, and of a cross formed by similar bits of paper pinned to the curtain in front of the subject. The length of the vertical line was 48 inches; the height of the cross was 54 inches, its width, 35 inches. Both forms were about ten feet from the subject's eye as he sat in the usual position in which the photographs were taken. Figure 55 shows the fixation of 5 disks of paper horizontally placed, the whole form measuring ten inches, the disks being 2.5 inches apart, from center to center, and the form being placed ten feet from the subject's eye. The subject, *Hof.*, examined these objects by fixating, one by one, the papers of which they were composed. For figures 53 and 54 he was allowed to move his eye as he pleased, as far as the time of each fixation was concerned; for figure 55 he changed fixation in time to my count, which called for a change about every two seconds. The prints from which these reproductions were made were enlarged to four times the size of the original negatives.

In figures 53 and 54 may be seen a slight skew of the forms towards the subject's left (the right eye was being photographed), which illustrates the distortion suffered by the path of the eye's movement, in the path described by the corneal reflection, to which Stratton calls attention. The distortion differs in amount and direction according to the direction of the eye's movement and the position of the light in relation to the eye (13, p. 340). Figure 55 gives an example of the inexactness of the eye's fixations, demonstrated by Stratton (13, pp. 342–344). The five disks in figure 55 were arranged in a straight line and hung before the subject, with extra illumination, so that he might see them without difficulty. Yet the fixations are not in line. Apparently there were two fixations for the last disk on the left, and neither one is quite in line. Stratton's drawings show that in following the outline of so simple a form as a rectangle, the fixations at the corners might be conspicuously out of position. These facts may be usefully kept in mind in interpreting the photographs of eye-movements, especially those of the cross. Figure 56 shows a steady fixation of ten seconds duration.

In the pages which follow, I shall report all experiments in which it was apparent that the subject actually experienced a visual image, with the exception of the few in which the small diagrams were used as objects. This means actually all the experiments in which photographs were taken (with the exceptions just noted).

Several photographs that I shall mention lack the head control. All but two of these were taken either at the very beginning of the work, when the problem of arranging a large enough surface of plate to accommodate both the corneal reflection and the head control had not been solved; or during a period of three days, when a different position for the subject was being tried out, which unexpectedly threw the head control off the plate. Since evidence of head-movement is so very rare in the photographs which do show the head control, the photographs which lack it are by no means valueless.

I have grouped the photographs in three classes, positive, doubtful and negative. The positive plates are those in which the fixations are grouped in accordance with the general spatial aspect of the object visualized; the doubtful ones show that eye-movements have occurred but do not show them in characteristic grouping; in the negative plates, the photograph is not substantially different from those taken during fixation.

The photographs of the corneal reflection are lettered A; those of the head control, B. Each figure, therefore, consists of an a-plate and a b-plate side by side.

It is not by any means always possible to determine the number of eye-movements that are represented in a photograph, but it is always possible in the positive and doubtful plates to see that several have occurred. When the forms of the corneal light overlap and the outlines of the individual fixations cannot be distinguished, frequently the projection of the horizontal arms of the cross shaped reflection, at the sides of the group of merged fixations, shows that more than one fixation has occurred.

It will be well to keep in mind, in examining the photographs, the fact that the reproductions have been made from prints enlarged to at least four times the size of the original negatives; in some cases the enlargement is more than four times. Since the camera gave an enlargement of about four times, the extent of the eye-movements as they appear in the reproductions are something like sixteen times the extent of the actual movements of the eye. In some instances the enlargement of the print is somewhat more than four times the size of the negatives.

The question of the possibility of voluntarily executed eye-movements having occurred in the case of subjects who knew the purpose of the experiments, has been raised by one or two persons who have seen the photographs. Aside from the honesty of the persons who acted as subjects, the strongest argument against this idea is to be found in the extreme unlikelihood that eye-movements of such very small extent could be voluntarily executed by so many persons with such similar results. If they had all seen photographs similar to those shown here and had practiced reproducing them, they might have succeeded, but of course the results of the experiments were not even discussed with any of the subjects. In the case of the imagery of the cross, also, certainly attempts to outline the form by moving the eyes voluntarily over the curtain would have given much more accurate shapes than the photographs show.

Unless otherwise stated in the descriptions of the photographs, the time of the exposures was determined by the time the subject took to complete his image, the closing of his eyes being the signal for covering the lens. Unless otherwise stated, the images were not projected.

VII. SUMMARY AND CONCLUSION

Table 1 gives in concise form the results of one hundred and five experiments, showing the number of positive, negative and doubtful photographs of each subject.

Of these, eight lacked the record of the head control, as follows: *Hof.*, one cross; *War.*, one cross, one train, two monuments slanting; *Re.*, two colored print. These were all positive plates except one doubtful "monument slanting," of *War.*

Head movement was recorded in six of the one hundred and five plates but in none did the movement of the head destroy the significance of the eye-movement record.

The results of the experiments seem to justify the conclusion that eye-movements, such as would be required in the perception of an object, do occur in visual imagery of that object. The nine negative and twenty-one doubtful plates make it impossible at this point to say that such eye-movements *always* occur. The doubtful plates do not constitute negative evidence, for in none of them is the grouping of fixations such that the characteristic grouping may not be included in it, disguised either by inexactness of spacing (see discussion of cross, p. 34) or by additional fixations which may have occurred before the appearance of the image or at random during the imagery. With a way of determining the temporal sequence of the fixations, a doubtful group could probably be analysed so as to determine its real nature and meaning, but under the conditions of these experiments they can only be labelled "doubtful" and left at that.

The negative plates offer the real problem. They may mean either of two things; (1) the conditions of the experiments may not have been adequately controlled, with the result that objects were not visualized as "nearby" and "large" and with attention directed to the various parts. In the case of four of the negative plates this seems probably to be the explanation. One negative plate from *Rit.*, (monument), and one from *Bas.*, (monument), were taken before the instructions included the requirement to see the monument "large." For one plate from *Re.*, of the monument, the report stated that he had seen it as if with one point fixated, and the rest in peripheral vision. As *K.* pointed out in connection with his imagery (fig. 2) it is difficult to state with certainty how large your image is. (2) The objects may have been actually visualized with all the characteristics of size and spatial relation which would require eye-movement in perception, but eye-movements did not occur. In this case either some other muscular mechanism may have been substituted for eye-movements; or an image may occur with all the spatial attributes of a perceived object, without any muscular change being necessary for the imaging of these attributes; or the muscular change may

TABLE 1

	POSITIVE	DOUBTFUL	NEGATIVE
Hof	15	4	0
K	3	3	2
Rit	6	1	1
War	19	5	1
Shil	7	1	0
Hi	6	0	1
Re	10	5	1
Bas	4	1	1
B	5	1	2
	75	21	9

be so slight as not to produce a change in position of the eye-ball, that can be recorded by photography, in which case the galvanometer would offer the only suitable method of study.

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EXPLANATION OF PLATES

Plate I

Figures 1 to 9 inclusive, show the corneal light and head control during visual imagery of the Washington Monument, for each of the nine subjects. The instructions were, as quoted before: "When I say 'go' please get a visual image of the Washington Monument seen as if nearby so that it will appear large. Be sure to see all the parts, base, shaft and top." In the later experiments the instruction was added: "Do not repeat the experience but close your eyes when you have finished." Every a-plate shows several fixations arranged vertically, with occasionally some additional fixations to one side or the other. Three of the b-plates indicate head movement but in no case does the head movement account for enough of the change in eye position to destroy the value of the a-plate. There is possibly head movement in a fourth.

Fig. 1. Positive. Subject *Hof*. Possible head movement accounting for the sideward shift that is to be seen in the a-plate. Exposure 7 seconds. Report: "I saw the circular railing, the pavement, base, shaft, landing at top, pigeon holes and Washington. Could only see one part at a time. I was standing about at the Lafayette statue." Five other photographs were taken of *Hof*. visualizing the monument seen large. Three of these (one, reproduced as figure 46, and one other were projected), show the vertical arrangement of fixations as strikingly as in figure 1. One gives eye-movements, but not characteristically arranged. Another, the first one of any sort taken of *Hof*., shows little movement; the introspections report his having imagined himself as walking about the Square, seeing everything in it—monument, statues, fountains, houses.

Fig. 2. Positive. Subject *K*. Head movement accounting for sideward shift seen in the a-plate. Exposure 5 seconds. Report: "I immediately experienced a rather clear image of the Washington Monument. It was moderately large and projected before me. The size seems to me largely a matter of meaning and therefore very difficult to describe. I should say that if projected upon a surface about ten feet before me it could be said to be about three feet high. The fixation point was near the top of the shaft at first. I seemed to take particular notice of the figure at the top and then there seemed to be a sort of sweep downward, which brought the whole shaft and base into comparatively clear 'view.' In this the whole monument seemed to be clear and distinct at the same time, more than in any previous observation. The imaginal content was a pattern of grays against a darker and wholly unformed background." This was the last of the experiments in which *K*. acted as subject. Some days previously I had urged him not to hurry through his imagery, but to take as much time as he wished. This had resulted in a greater feeling of ease during the experiments. Three other experiments in which *K*. was asked to visualize the monument as large and detailed, gave two photographs with scarcely more movement than would occur in fixation (negative), and one doubtful. For one of the negative photographs, the introspective report states that the monument had been visualized with one point fixated and the remainder in peripheral vision. In all these cases the imagery was unsatisfactory to the subject in the matter of clearness.

Fig. 3. Positive. Projected. Subject *Rit*. No head movement. Exposure 15 seconds. The directions had required that the figure of an angel be substituted

for that of Washington. Report: "I was standing to the North of the monument. Saw the fence with cross bars, the base rather hazily, shaft clearly, the little balcony around the top; the angel was facing East; with large wings. It was pretty much the angel from a statue in the Corcorna Gallery in Washington. It had the hands in position to hold something but I didn't see anything." There were two other experiments in which this subject was required to image the monument as large and detailed. One gave eye-movements characteristically placed. The other (the first taken of *Rit.*), was taken before the instructions were made specific as to seeing the monument "large," and shows no movement.

Fig. 4. Positive. Subject *War.* Head movement accounting for sideward fixations seen in the a-plate. Exposure 10 seconds. Report: "I was standing on the North side. Trees in bud and grass coming out. I think I got it as soon as you said 'go.' It came all at once, not piece by piece. I saw the church at the side, it seemed like" In answer to my question, "What details of the monument did you see?" he said "Balcony around the top I noticed most." This subject's introspections were apt to be scrappy and questions didn't often bring out much more. Six other photographs were taken of *War.* visualizing the monument as large. Three of these show the same vertical arrangement of fixations (one is slightly oblique). Two are doubtful. The sixth photograph shows virtually no movement.

Fig. 5. Positive. Subject *Shil.* No head movement. Exposure 17 seconds, determined by experimenter. Report: "Saw the base, then the shaft rising up and Washington at the top. Think I saw the small square base under the statue. Saw the entrance to the base." Three other photographs were taken of *Shil.* visualizing the monument, and all show vertical arrangement of fixations and no head movement.

Fig. 6. Positive. Subject *Hi.* No head movement. Exposure 15 seconds, set by experimenter. Report: "Saw it from right close by. Saw the iron fence and inscription 'Commander in chief of the army.' Could see writing of dates. Then square block, and round shaft comes out of that square block to another square block and a man's figure at the top of the last square block. Just got up to the top when you stopped." No other photographs were taken of *Hi.* visualizing the monument.

Fig. 7. Positive. Subject *Re.* No head movement. Exposure 12 seconds. Report: "Saw the monument for the first time in gray stone. Saw the circular pavement, iron railing, square base and steps on the side, platform on top, the dome and the old boy himself. Think I finished seeing it a little before you put the cap on. I always manage to use all the time you give me, though; I go over it again, I didn't go over it again this time but I was taking particular pains about that top." Three other photographs were taken of *Re.* visualizing the monument "large." One of these shows characteristic vertical arrangement of fixations without head movement, and reproduced as figure 48. The third has pronounced eye-movement but is capable of more than one interpretation, therefore "doubtful." The third is negative but the subject reported that he had visualized the monument "small."

Fig. 8. Positive. Subject *Bas.* Head movement accounting for topmost fixation in a-plate. Exposure 15 seconds, determined by experimenter. Report: "I began at the bottom. Saw the curb and colored pavement stones, stone base

iron railing, steps to entrance, iron urns, doorway, square base, balcony, shaft with its little square porch, a little more shaft and the angel. It was facing the same way as the statue. One hand was stretched out and its wings were great big things, not outspread but sweeping down behind. The angel was white, while the monument seemed gray. It was a Greek angel because it had drapery." The requirement had been to 'image' the figure of an angel in the place of that of Washington. This image was projected. Two other photographs were taken of *Bas.* visualizing the monument. One shows characteristic vertical movements of the eye; the other shows virtually no movement, but was taken before the instructions were made specific as to seeing the monument "nearby and large."

FIG. 9. Positive. Projected. Subject *B*. No head movement. Exposure 13 seconds. Report: (omitting introspections on the foreperiod). "Strain about the eye-balls, strain of fixation, and word 'fixation' in some way. Then, 'no, get image.' Visual image then of the monument projected at the distance of the black curtain. First the lower part clear, with verbal recall of the word 'base'; then central part of shaft clearer, bright gray as if with the sun shining on it. Then kinaesthesis of eye strain and word 'fixation.' Fragmentary visual imagery of whole experimental situation; then the words 'mustn't fixate.' Then the top of the monument clearest. All this time the instruction 'large' was marginal in consciousness in auditory terms. The figure at the top was definitely outlined. That image went, and a small one came in of the monument as a whole, as if from Charles and Madison streets—very clear in bright gray as if the sun were shining on it from the West; it stood out against blue sky with white clouds. With this small image and some surroundings there was decided pleasantness and general experience of relaxation. Then I closed my eyes. When I was looking at the central part of the shaft there was a definite attempt, though unconscious, to inhibit eye-movements. The first large image was not as clear as the small one; the small one flashed in very clear." Another photograph was taken of this subject visualizing the monument as large. This shows little more movement than is apparent in a photograph taken of her while fixating a dot, and this movement is not in the vertical direction. In this experiment the subject was confused during the taking of the picture by recollections of images that occurred during the fore-period. Her report for the main period is, "Then I think, but am not sure, that with 'ready, go,' the earlier images vanished and an image came as if I were standing a little further away than in the second; the bottom part and shaft stood out in succession and the top with figure outlined against the sky stood out against the sky. Sorry I can't give more details than that."

PLATE II

Figures 10 to 18 inclusive, show the corneal reflection and head control of the same nine subjects during the visualization of a freight train standing still. The instructions were, "When I say 'go,' please get a visual image of a freight train standing on the track. See it as if from near by, so that it will appear large, and see all the parts; engine, coal tender, freight cars and caboose"; and later, "do not repeat the experience but close your eyes when you have finished." All the a-plates show horizontal arrangements of fixations, strikingly different from those involved in images of the monument. There is no head movement.

FIG. 10. Positive. Subject *Hof.* No head movement. Exposure 10 seconds determined by experimenter. Record: "First thing, I saw the freight car in front of me, with a hobo sitting on it. Then my sight jumped to the caboose and I saw the number 7489; there was a man reclining in the caboose. Then I came back to the same car with the hobo; then my sight went to the front end of the train, saw coal tender and man shoveling coal, engineer and locomotive. Saw these last in a hurry, because I spent most of the time on the other end of the train." No other photographs were taken of this subject visualizing the train.

FIG. 11. Positive. Subject *K.* No head movement. Exposure 26 seconds. Report: "I experienced a fairly vivid visual image of the locomotive of a freight train. The image was that of a view of one side of the engine. It was as though I were standing about 15 yards from the track. The large drivers of the wheels were most focal at first then there was a series of shifts of the imaginal pattern which brought the upper part of the engine, the smoke-stack, then the fore part of the engine and 'cow-catcher' to the focus, while the other parts were very obscure. There was the image at one moment of escaping steam. The imaginal qualities were grays. . . . After looking over the engine, imaginally, my imaginal gaze shifted to the coal car. I had a fairly definite visual image of the top part of the car. Then my imaginal vision shifted along the cars. It was as though I were standing where I did while 'seeing' the engine, so that my imagery of the cars and rear end of the train corresponded to the view one would get if one stood opposite the engine a few yards from the track and looked down the track toward the caboose. At first I did not get clear imagery of cars. Then I thought to myself, 'Get a clear image of some of the cars; note what kind they are.' Then I had rather a clear visual image of some box cars. But these box cars seemed to be detached from the train. For a moment the image of the train seemed to dissolve. The box cars were on another track, but this track was not in the same 'picture' with the former. It was another imaginal structure altogether. With a feeling of effort I again brought up the image of the train with the rear part most focal; the rest was very hazy and not definitely formed. The top part of the caboose was most focal and definite just before I closed my eyes." It was a day or two before this experiment that I had impressed upon *K.* the fact that he need not hurry his imagery. One other photograph was taken of *K.* visualizing the train and shows a similar horizontal arrangement of fixations.

FIG. 12. Positive. Projected. Subject *Rit.* No head movement. Exposure 15 seconds, determined by the experimenter. Report: "Rather hazy to-day, but the engine was towards my left and the coal-car, black, came next, with printing on the side. A red freight car and red caboose. It was standing on a track on a high bank of cinders or ashes." No other photographs or *Rit.* visualizing the train were taken.

FIG. 13. Positive. Subject *War.* No head movement. Exposure 7.5 seconds. Report: "From left to right, engine, coal tender, oil car, box car, caboose. Distance about seventy-five feet." Two other photographs were taken of *War.* visualizing the train, both showing the horizontal arrangement of fixations. In one the head control is not visible.

FIG. 14. Positive. Subject *Shil.* No head movement. Exposure 13 seconds, determined by experimenter. Report: "I confined my attention mostly to the engine, coal tender and caboose. Went over the freight cars rather hastily.

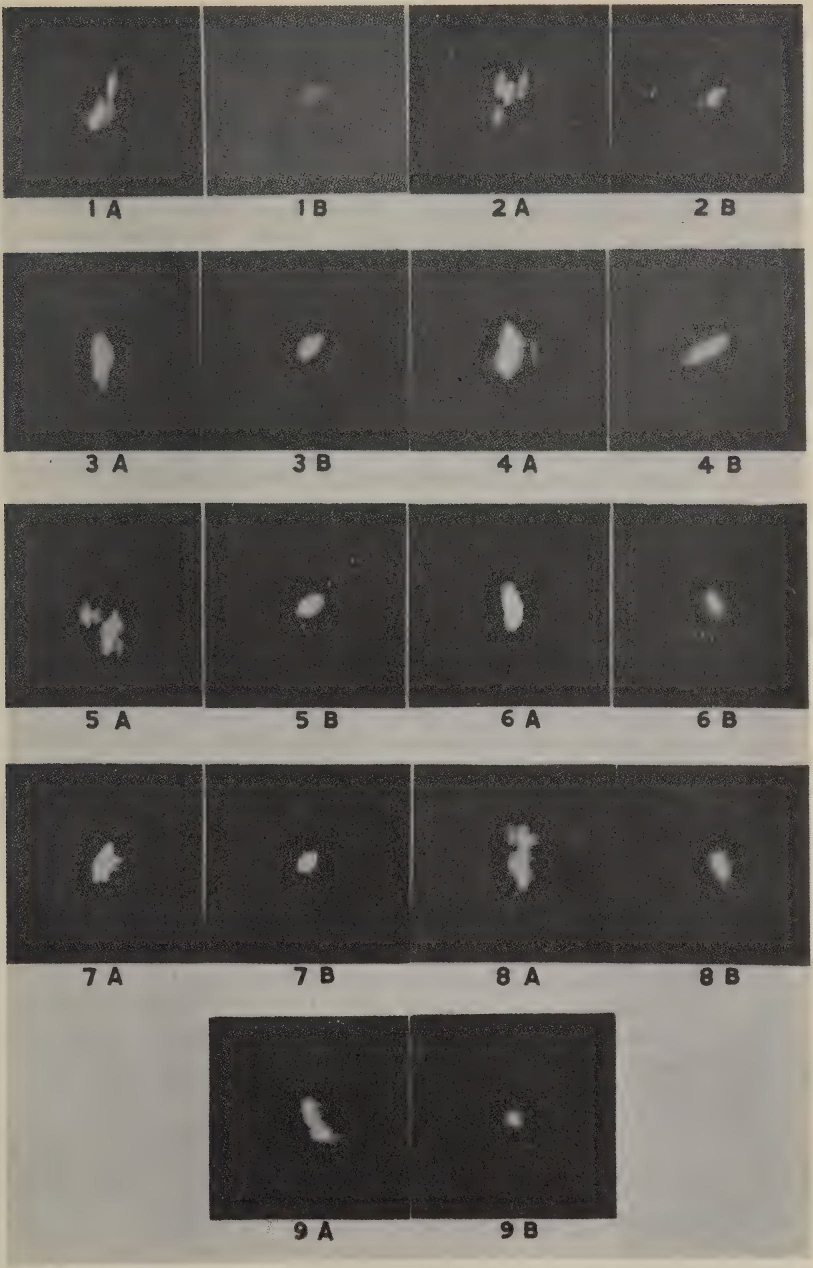


PLATE I

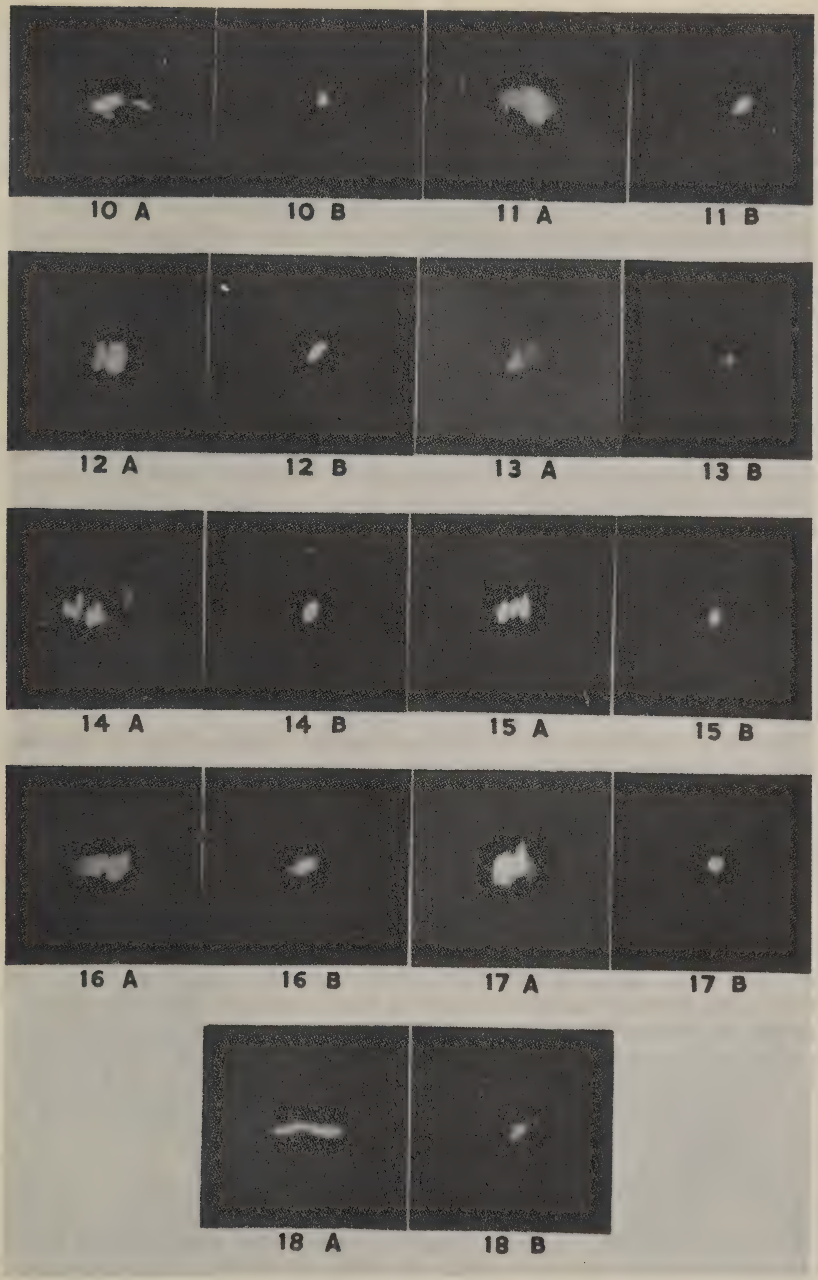
Didn't see the freight cars so plainly. There was a space between the coal tender and caboose. The caboose was on my right. The engine had a round smoke stack and large wheels, four on one side. The coal tender was an open car. The caboose was an enclosed car. Saw nothing in the surroundings. Time was a little short." There was no other photograph of *Shil.* visualizing the train.

Fig. 15. Positive. Subject *Hi.* No head movement. Exposure 19 seconds. Report: "Saw the engine, coal tender, long string of box cars, man walking on top of one of them, then all the way down to caboose. There was a man sitting on the back platform with his feet on the railing. I was standing about one third of the way from the front of the train, fairly close to it." One other photograph of *Hi.* visualizing the train is negative.

Fig. 16. Positive. Subject *Re.* No head movement. Exposure 14 seconds. Report: "I was standing opposite the engine. First saw the smoke stack, engineer's cabin, red glare of fire, coal tender, box cars (yellow), long train. I got each car separately. Red caboose in the distance; had some trouble getting it clear, because it was so far away. Got three colors, glare of the fire, yellow cars and red caboose." One other photograph of *Re.* visualizing train. Shows horizontal arrangement of fixations.

Fig. 17. Positive. Subject *Bas.* No head movement. Exposure 13 seconds, determined by experimenter. Report: "Started with the engine; coal tender, a yellow car, couldn't make out the letters on it. Then there was a white car, another yellow car foreshortened—the train was sided on a curve. Then the caboose—a little red caboose. Saw the machinery in the locomotive pretty thoroughly—brass fixings and a number 3 on the number plate on the middle of the boiler. I was right startled to see there was smoke coming out of the chimney and steam out of the sides. The engineer was in the engine, leaning on his elbow in the cabin. Saw the different rods and rails and wheels. Funny I should see it on a curve and perfectly foreshortened. Could even experience the difference in size between the front and the rest. How awfully immense the front of the engine seemed. And I could even remember the color of the engineer's overalls—blue and white." No other photographs of *Bas.* visualizing the train.

Fig. 18. Positive. Subject *B.* No head movement. Exposure 7 seconds. Report: "With, 'Ready, go,' a little further off (then the preceding image of the fore-period), not so large, the engine first, clear; then window in the cabin and engineer looking out with elbow on the edge. Then kinaesthesis in eyes as if I were looking down the train, saw cars and windows, then front of second car. Then vocal-motor-auditory, 'freight car,' in your voice. Then, 'Oh, all right.' Cars changed into freight cars; word caboose came and a small car came. Just after the freight cars appeared, I was conscious of a slight eye kinaesthesis. Flashing fragmentary imagery of the experimental situation here, carrying the whole meaning 'Eye-movement; I mustn't try to fixate.' Closed my eyes." One other photograph of *B.* visualizing train standing still, shows the same long horizontal arrangement of fixations. An interruption occurred before the introspection was taken and the subject was too uncertain of her recollection to be willing to give a report on this experiment.



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12 A 12 B 13 A 13 B

14 A 14 B 15 A 15 B

16 A 16 B 17 A 17 B

18 A 18 B

PLATE II

Plate III

Figures 19 to 27 inclusive, show the corneal light and head control during visual imagery of a cross by each one of the nine subjects. In some instances it was an imaginary "flaming cross," in others, a wooden cross which was exhibited to the subject before the experiment. The instructions were: "When I say 'go,' please get a visual image of a flaming cross (or the wooden cross) seen as if from nearby, so that it will appear large. Be sure to see all four extremities. Do not repeat the experience but close your eyes when you have finished.

The cross is a much more complicated figure to deal with than are the other objects used. The task of getting the four extremities in the correct spatial relations one with another, when experienced in succession, would be far less easy of accomplishment than that of getting the spatial relations of successive parts of a straight horizontal or vertical object. The subject may say, as some did, "I went up to the center, out the left arm, then back to the center etc." but we must remember that, after all, he was not dealing with an objectively fixed center that he could look at, leave and return to, but that he simply created a center, so to speak, and then later created another one. The chance that the second one would be related to the rest of the picture exactly as was the first, or that directions and distances would be managed accurately, seems very slim. Because of these difficulties of the task, the tendency inadvertently to include some parts in peripheral vision while "looking at" other would be less easily controlled than with the simpler forms.

In the light of these considerations, a photograph showing an accurately outlined cross would rouse in one's mind, suspicion of fraud. It is not surprising that none such is found among this group of photographs. Nor is it surprising that the cross yielded a much larger proportion of photographs that are to be classified as doubtful, than did the other objects.

Figures 19, 22, 24 and 27 are positive, the rest doubtful. Of the doubtful plates, Figure 20 presents a series of horizontally placed groups of fixations in no way suggesting a cross. Figures 21, 23, 25 and 26, however, are readily seen to consist, each, of three groups of fixations, a central one with another at either side, not symmetrically placed but highly suggestive of the cross shape, with directions and spacing not accurately arranged.

There is no head movement apparent in any one of the b-plates.

Fig. 19. Positive. Subject *Hof.* No head movement. Exposure 10 seconds. Report: "I started from the bottom and went to the top, then the center, and to the right; center and left; back to the center and down again. I tried to find those figures but only found two—the top, black rectangle with white triangle, and left, the Turkish moon. This was nearer to me than the flaming cross—it was as near to me as you held it. Just got the color of the wood; the two figures were black and the white triangle was white against the black." This was the wooden cross. *Hof.* reported that it was easier to image than the flaming cross, and all show groupings very similar to this. In one of the three the head control is lacking.

Fig. 20. Doubtful. Subject *K.* No head movement. Exposure 14 seconds. Report: "I visualized a rather large cross on top of a small hill. I was near the foot of the hill and my imagery corresponded with a view I should get if I were looking up this small hill at the cross. My image of the cross was a disappointment in that I was quite unable to visualize clearly any red flames streaking up from its arms. Before the exposure (?) period began and while the instructions were being given, I had a good clear image of a cross with the cross bar in the normal place and with the tongues of flame leaping up from them. Now I was annoyed by a distortion of the image which I am wholly unable to explain. The cross bar was below the center of upright timber. This was disconcerting and with an effort to put the bar in its normal place higher up, came a sort of deterioration of the whole visual pattern into just a confused mass of grays. Then I thought 'Well, it's an image I had all right, so why not let it go and describe it'. Then I closed my eyes. Around that cross bar there was wrapped a lot of stuff that appeared like waste or old rags. This cotton waste and string were visualized in terms of imaginal grays. Above this there appeared bright and flashing forms which meant flames. These forms were very light silver-like grays with only a slight tinge here and there of pink. I felt disappointed because they did not appear red as when I first visualized the cross before the observation period. I visualized the different extremities of the cross but the left side of the bar was the only part that was vivid and definite in contour. The rest was rather hazy." In analyzing Figure 20 it must be remembered that each of the large white spots in the photograph represents several fixations vertically placed. One other photograph was taken of *K.* visualizing the cross with results similar to this.

FIG. 21. Doubtful. Projected. Subject *Rit.* No head movement. Exposure 10 seconds, determined by experimenter. Report: "One piece reached from clear at the top of the curtain to about a foot from the bottom. The other was about the same length. Flames were about 2 or 3 inches long; in color, dancing up and down. The cross was resting above something black and there were no other objects in the picture." Image was projected. No other photographs of *Rit.* visualizing cross.

FIG. 22. Positive. Subject *War.* No head movement. Exposure 8 seconds. Report: "I looked from bottom to top, back to cross and then from left to right. I was about 25 to 50 feet away. Just a flaming cross and no background." Two other photographs were taken of *War.* visualizing the cross, one positive, (lacking head control), one doubtful.

FIG. 23. Doubtful. Subject *Shil.* No head movement. Exposure 13 seconds determined by experimenter. Report: "First imagine cross without flames, and then imagine flames. It was hard to get the flames. Saw it right in front of me, about two feet away. Saw the upright first and then the cross. The time was all right." No others were taken of the cross with *Shil.*

FIG. 24. Positive. Subject *Hi.* No head movement. Exposure 16 seconds set by experimenter. *Hi* had once witnessed a Ku Klux demonstration and reproduced the scene here. Report: "I saw it. Started at base. Saw a few Ku Kluxes at the bottom, then straight up the middle, the main body of the cross, to where the cross bar is nailed on, then went to the left extremity, then went to right. Did not go clear to the top, the place where the cross bar was nailed on was near to that. The time was just about right." One other photograph of the cross (the wooden cross) was taken with *Hi.* and shows a scattering of fixations very similar to this.

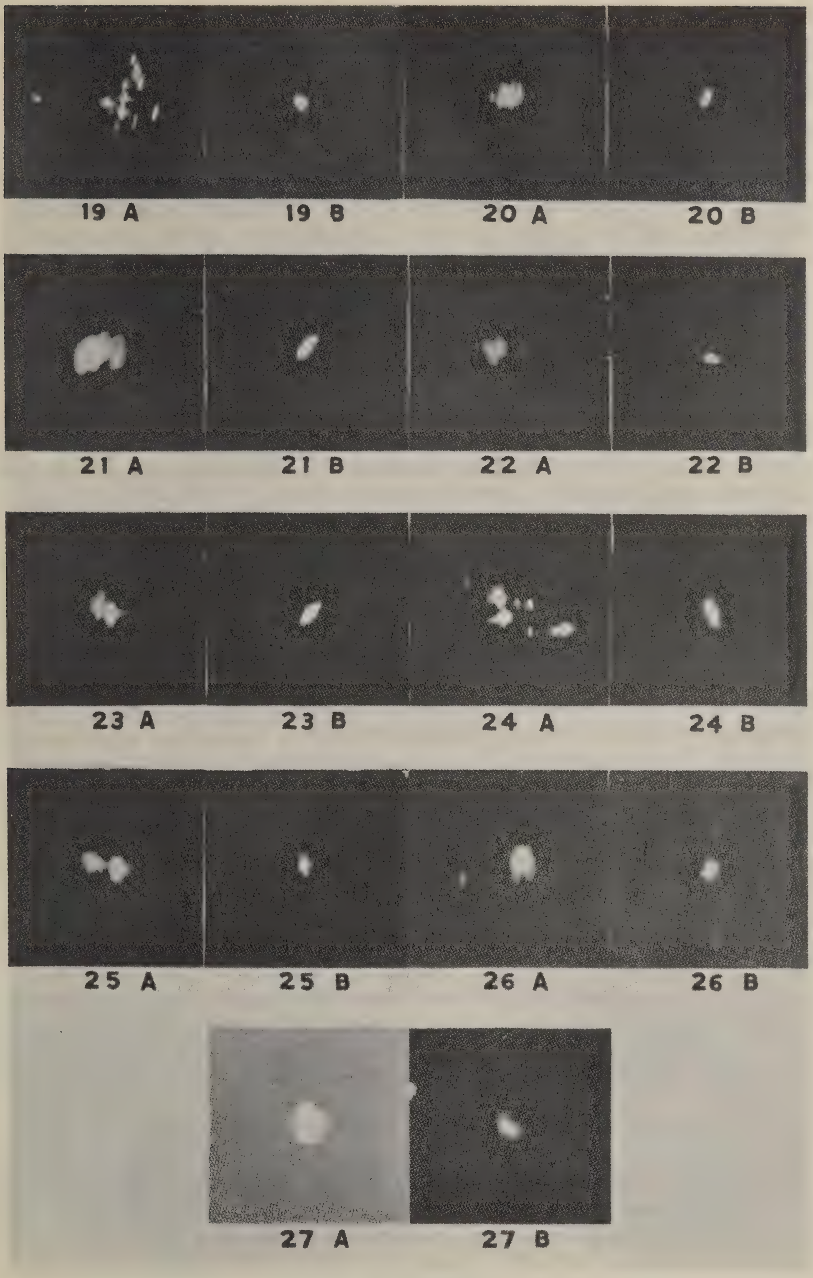
FIG. 25. Doubtful. Subject *Re.* No head movement. Exposure 20 seconds. Report: "First saw entire cross about the size as it stood, not particularly distinct, just got the shape. Then I got each corner, first right, then left, top and bottom. Got each design in respect to the board it was on, then got the cross over again, entire and got the position of the boards, the horizontal one back of the perpendicular one with the nails holding it together. Saw the boards white." I see from my notes that I altered the wording of the instructions for this experiment, saying, "When I say, 'go', get a visual image of the cross from top to bottom, or from bottom to top and from one side to the other, see it large and clear and close your eyes when you have finished." One other photograph of the cross with *Re.* shows a general configuration somewhat similar to this, but so crowded that it is not possible to distinguish the fixations. It was the flaming cross which he saw, "not very large." I have classed this record as doubtful.

Fig. 26. Doubtful. Subject *Bas.* No head movement. Exposure 15 seconds, set by experimenter. Report: "Rather simple, easy to do. It was a square-timbered thing, burning on the front surface and flames sweeping back to right. Saw the two arms and upright. Saw the color, yellow and orange and green of the flames. Time was all right." Projected. No other photographs of *Bas.* visualizing cross.

Fig. 27. Positive. Projected. Subject *B.* No head movement. Exposure 5 seconds. Report: "In the fore period, after the instruction, I had a visual image of a cross, artificially flaming, localized on the curtain in front, not very large. Then you said 'near at hand' and there was kinaesthesis as if changing the focus, and strain in the head and neck and image of cross there. In final instruction 'don't get a second picture' there was the vocal motor, 'May not be able to help it'. With 'Now', a repetition of the first cross not very large, out there on the curtain. It was plainly visible as a whole. I am not sure of the coloring. I think there was the dark brown of the wood with lines streaming up from it. There was recall of the instructions 'as large as possible'. Then I think there was a momentary flash of a larger cross, then the smaller cross returned and remained. Then I closed my eyes." It shows a great many fixations arranged in three groups being placed very much as are those of figure 25, and suggesting the cross with directions and spacings inaccurately arranged. It is to be classified as doubtful.

Plate IV

Figures 28 to 34 inclusive, show the results of visualization by seven of the subjects in response to the instructions: "When I say 'go', please get a visual image of two men sitting at opposite ends of a long table. See them as if you were seated at one side of the table, with one at your right and one at your left." All of the subjects except *B.*, spoke of the impossibility of seeing two men simultaneously, in such positions, and of the necessity of seeing first one and then the other. This was acknowledged and they were told it was all right to see first one and then the other. Figures 28, 30, 31 and 33 are obviously positive, showing two groups of fixations with an interval between. Figure 34 is quite as obviously negative. Adhering strictly to the terms of my classification, I have included figure 29 and figure 32 in the doubtful list because the fixations form a continuous horizontal configuration, although this configuration is entirely appropriate to the visual experience of the two subjects as described in the introspections. In



19 A 19 B 20 A 20 B

21 A 21 B 22 A 22 B

23 A 23 B 24 A 24 B

25 A 25 B 26 A 26 B

27 A 27 B

PLATE III

figure 29, *War.* saw the wall paper, and a Hopkins pennant hanging between the two men, and also repeated the observation of one and perhaps both of the men. It could hardly be expected that a second "glance" at either man would bring the eye to exactly its earlier position and the expected result of the whole experience, supposing that a fixation resulted from the imaging of each part of the picture, would be a series of horizontally placed fixations with a rise above the general level corresponding to the imaging of the pennant. In figure 32, *Re.* repeated the observation of the two men and the photograph shows four groups of fixations.

Fig. 28. Positive. Projected. Subject *Rit.* No head movement. Exposure 13 seconds set by experimenter. Report: "Rather plain. Table unpainted wood with man at the left leaning back and smoking. Man at the right leaning forward, both hands lying out on table. May have been talking but I didn't hear him say anything. The time was all right." Projected. No other photograph of this subject visualizing two men.

Fig. 29. Doubtful. Subject *War.* No head movement. Exposure 13 seconds, set by experimenter. Report: "A white table cloth. One at right in light brown suit, a great big fellow. One at left smaller and in light brown suit. Wall paper was same color as the suit. Directly opposite me was a Hopkins pennant, black and blue. A window in the wall at right. Saw the man on the right first and then the one on the left, and then went back to the one on the right. Don't know whether I saw the one on the left again or not." No other photograph of this subject visualizing the two men.

Fig. 30. Positive. Subject *Shil.* No head movement. Exposure 13 seconds set by experimenter. Report: "Both seemed to be the same as far as clothes are concerned. Saw the one on the right first and then the other. Both had stiff collars on and four-in-hand ties. Saw nothing between the two." No other photograph of this subject visualizing the two men.

Fig. 31. Positive. Subject *Hi.* No head movement. Exposure 25 seconds. Report: "Saw man at left first, noticed he was scratching his face and saw the way he ate, watched his movements. Then noted the man on the right; he was talking to me; once in a while he would smile." The three fixations in the photograph, at the reader's right, are in the position of the subject's left. The man on the left was the one so carefully observed. No other photographs of *Hi.* visualizing the two men.

Fig. 32. Doubtful. Subject *Re.* No head movement. Exposure 15 seconds set by the experimenter. Report: "Came right off in about three seconds. Did it twice and was doing it the third time when you stopped. It was easy because I imagined myself at the Barn, seated at a long table as I am every day, and I saw two of my companions, one at my right and one at my left. I recognized the two fellows I was looking at." No other photograph of *Re.* visualizing two men at table.

Fig. 33. Positive. Subject *Bas.* No head movement. Exposure 13 seconds, set by the experimenter. Report: "Saw the man on the right first and then the one on the left. The one on the right had on a light suit. There was a pad on the table. He was leaning back with his hands on top of the table, at the edge. The man on the left was in a dark suit, leaning over the table, with his arms on the

table. He had dark hair. The other fellow had light hair." No other photographs of *Bas.* visualizing the two men.

FIG. 34. Negative. Subject *B.* No head movement. Exposure 5 seconds. Report: In the fore period the subject had a succession of images, as result of the preliminary explanation of what the task was to be, with finally this: "Then the same image of the table came in front of me as if I were manipulating the image. I was seated at the table with my back to the window, very clear and detailed. Then vocal-motor 'That's it.' Then came the final instructions; the image became more focal, so that at 'ready, go,' it was very focal, color clear. Miss B. at my side, B. beyond and L. at the end. Then at my right Miss ——. I said, 'No, man.' Then saw B. clearly at the end of the table. Vocal-motor 'B.' Same realization of myself sitting there. Kinaesthesis of my position here, formed nucleus around which the image formed. The right hand end of the table with L. was not so clear as the succeeding image of B. During the transition from B. to L. there was consciousness of my own eyes—can't say whether it was strain or image of eyes, or vocal motor, 'Am I getting image without turning eyes from one end of the table to the other?' Then I closed my eyes." No other photograph of *B.* visualizing two men.

Figures 35, 36 and 37 show the corneal reflection and head control during the visualizing of the Washington Monument, seen in horizontal position. All of these experiments were preceded by practice (without photography) in visualizing the monument, successively, in the vertical, oblique, and horizontal positions, with emphasis upon the point of getting clearly the differences between the positions. Only *War.*, *Hof.* and *Re.* were given this task (the monument in horizontal position), the other subjects having left town for the summer vacation.

FIG. 35. Positive. Subject *War.* No head movement. Exposure 10 seconds. Report: "Lying West, that is the top was to the West of the dome. I saw all the parts in order but when I came to the top, the platform obscured the dome. Standing on the North side of Monument street near Charles. Saw the base right in front of me and the rest up Monument street. Saw green grass and white cement walks. Not foreshortened particularly." No other photographs of *War.* with this 'imagery'.

FIG. 36. Positive. Subject *Hof.* No head movement. Exposure 13 seconds. Report: "The circular pavement around the monument was raised off the ground and the railing formed a circle around the monument. Then came the base, shaft, landing at top, then Washington. A support seemed to drop from Washington's head to the ground. "The head of the statue lay to the subject's right, the circular pavement, on edge, to his left." One other photograph was taken of *Hof.* during this imagery, and shows fixations in slightly oblique position. This last mentioned image was projected.

FIG. 37. Positive. Subject *Re.* No head movement. Exposure 18 seconds. Report: "It was horizontal but it wasn't resting on the ground. The circular fence was encircling the base in its horizontal position. The base had the door. The first platform, shaft, second platform, dome and monument. The hand was towards the ground. The head of the monument was towards my right. Got it without any trouble but didn't bother with too much detail. It kept its horizontal position." No other photographs of *Re.* with this 'imagery'.

Plate V

Figures 38 to 41 inclusive, show the corneal reflection and head control for *War.*, *Hof.*, *Re.* and *Hi.* while visualizing the Washington Monument seen as if *slanting* at an angle of 45 degrees.

Fig. 38. Positive. Subject *War.* No head movement. Exposure 12 seconds. Report: "Saw it from N. W. corner of Monument and Charles streets and it was raining. It was leaning towards me, sort of at an angle, for half of it, then continued straight across in front of me, yet I didn't see any bend in it. It slanted towards my right." Four other photographs were taken of *War.* visualizing the monument in this imagined position. Two of these show the fixation of *horizontal* position, two show them *obliquely* arranged. In two of these four the head control is lacking. Before the experiment represented by figure 38 and one of the horizontal photographs, the subject was asked to get in succession, images of the monument in upright, oblique and horizontal positions, and to be sure that he got clearly the differences in the position.

Fig. 39. Positive. Subject *Hof.* No head movement. Exposure 11 seconds. Report: "The circular pavement rose from the ground and the rest adjusted itself to that—base, shaft and top. This time, too, a support appeared under Washington's head. (Compare with Report, figure 36.) The distance from Washington's head to the ground was equal to the distance from the base to the lower end of the support, making a right angle triangle. It slanted to the right." Three other photographs were taken of *Hof.* visualizing the monument in oblique position. Two show the oblique arrangement of fixations slanting to the subject's right as the introspection describes it. The third is a complex arrangement of fixations that is to be classified as doubtful.

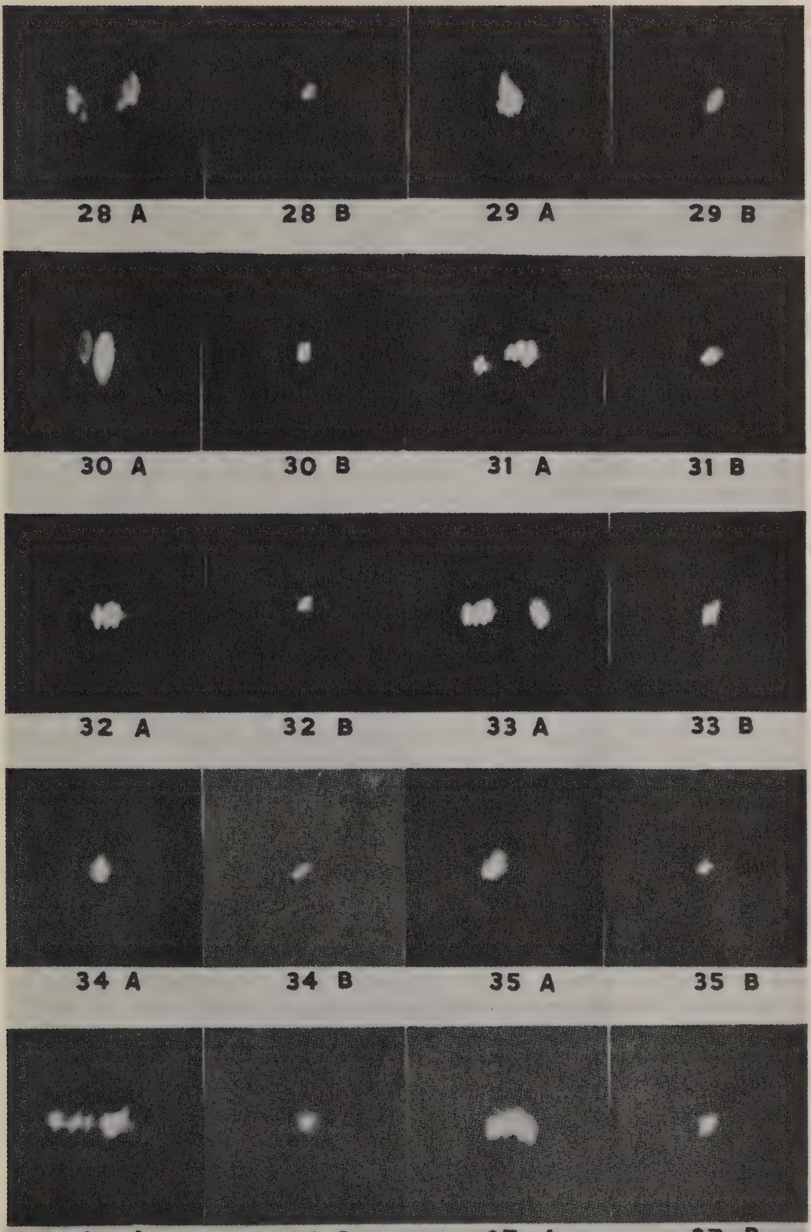
Fig. 40. Positive. Subject *Re.* No head movement. Exposure 24 seconds. Report: "The iron fence was on the ground and only the column was at an angle. Slanted to the right." Two other photographs were taken of this subject visualizing the monument in this position. One shows horizontal placing of fixations, the other, *oblique*. This subject was also practised with the monument in the three positions, before the taking of the two photographs which show the oblique position.

Fig. 41. Positive. Subject *Hi.* No head movement. Exposure 15 seconds set by experimenter. Report: "Started at bottom and saw the square base tilted so that the left side seemed higher than the right. Saw the middle of the shaft and went up to top. It was slanting towards the Methodist church. Saw the figure, the head was prominent against the blue sky. Then came about half way down again and went back." In this experiment *Hi.* was asked to see the monument slanting towards the right. No other photographs of visualizing the monument in this position.

In the four photographs reproduced, and in those not reproduced, in which the oblique arrangement appears, the group of fixations slants towards the subject's right, and in the introspective report in every case the monument is described as slanting to the right.

I think it is important to note that in all experiments in which the monument was to be visualized as slanting, and in which the oblique position of fixations does not appear in the photographs, the position is *horizontal*, never vertical.

Figures 42, 43, 44 and 45 show the corneal reflection and head control during



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30 A 30 B 31 A 31 B

32 A 32 B 33 A 33 B

34 A 34 B 35 A 35 B

36 A 36 B 37 A 37 B

PLATE IV

visualization of a moving object. I shall quote only portions of the introspections corresponding with these photographs.

Fig. 42. A moving train. Positive. Subject *Rit.* No head movement. Exposure, time not accurately taken; about 12 seconds; determined by experimenter. Report: "The train was moving rather rapidly and gradually turned towards me. It got to the other side of the screen by the time the cap was replaced." Image was projected. No other photographs of *Rit.* visualizing moving object.

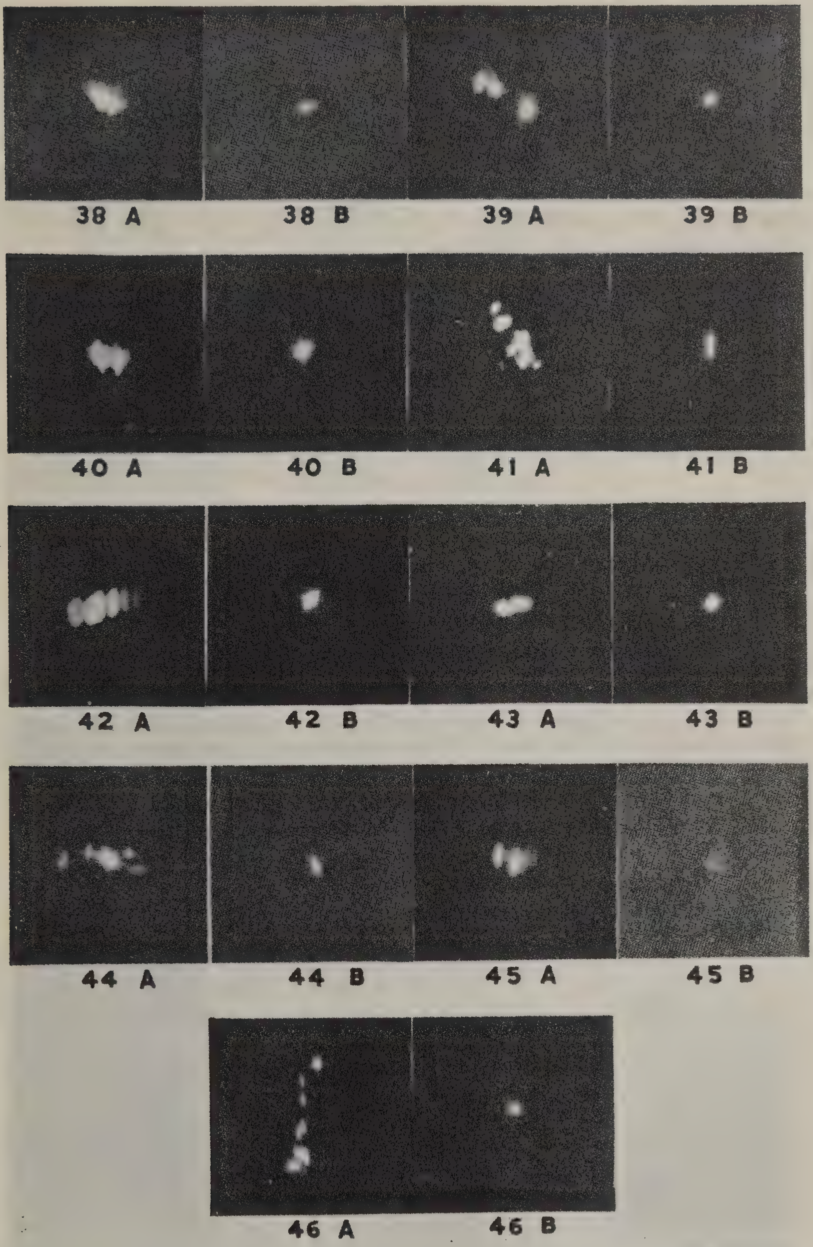
Fig. 43. A moving train. Positive. Subject *B.* No head movement. Exposure 11 seconds. Report: "One of the former images returned (i.e. one of those of the fore period) an image I had discarded as too far away, came; of a train, engine and number of cars. Came as a whole. Recalled instructions in your voice 'see details and see it moving'. Then the engine stood out more clearly with cab window with man leaning out, another man somewhere, then the rest less clear and detailed. Then a succession of cars and as last end of last car got in front of me, (vocal-motor) I forgot to count them. Then as if I turned my head to the left, with another image and vocal-motor-auditory 'one, two, three'. Then 'I don't believe that was right'. In counting there came something meaning 'If I have the image I ought to be able to count them'. The train was going from right to left with smoke streaming back to my right." No other photographs of this subject visualizing moving object.

Fig. 44. A moving locomotive. Positive. Subject *Hof.* No head movement. Exposure 5 seconds. Report: "Saw it coming from the right. Was standing near the opposite track; had a fine view of it. It passed me, saw it approaching and then in front and then past me." No other photographs of this subject visualizing moving object.

Fig. 45. A moving automobile. Positive. Subject *War.* Head movement uncertain. Report: Gray or blue automobile with black top. Went by about three feet away from me, going very fast." Two other photographs were taken of this subject visualizing moving objects, (train and automobile), both positive.

Plates V and VI

Figures 46 to 51 inclusive, show the corneal reflection and head control in three pairs of experiments, the purpose of which was to contrast the eye-movements in visualizing the monument as nearby and large with those occurring in imaging it as far away and small. The three subjects again were *Hof.*, *War.* and *Re.*, the only ones still in town when the experiments were performed. In order to avoid, as far as possible, altering the size of the group of fixations through an alteration of focus, the support fastened about the subject's head was left in place during the interval between the two experiments of a pair. The mouthpiece was removed in order to permit giving the introspective report. A very tiny amount of re-focussing was necessary in each case, but in Figures 46, 47, and 48, 49 the differences in size are clearly due to the fact that there were a greater number of fixations when the monument was visualized as large, than when it was visualized as small. Figure 50 is unfortunately so indistinct that a comparison of it with 51 is not very impressive. There is nothing of special importance in the introspections obtained in these experiments.



38 A 38 B 39 A 39 B

40 A 40 B 41 A 41 B

42 A 42 B 43 A 43 B

44 A 44 B 45 A 45 B

46 A 46 B

PLATE V

First Pair:

FIG. 46. Monument visualized as nearby and large. Positive. Subject *Hof.* No head movement. Exposure 7 seconds. Image projected.

FIG. 47. Monument visualized as far off and small. Positive. Subject *Hof.* No head movement. Exposure 5 seconds. Image projected. One other pair of photographs were taken of *Hof.* visualizing the monument in the same way, except that the images were not projected. They show equally striking contrasts in extent of fixation pattern.

Second Pair:

FIG. 48. Monument visualized as nearby and large. Positive. Subject *Re.* No head movement. Exposure 16 seconds.

FIG. 49. Monument visualized as far off and small. Positive. Subject *Re.* No head movement. Exposure 10 seconds. No similar pair of experiments were tried with *Re.*

Third Pair:

FIG. 50. Monument visualized as nearby and large. Positive. Subject *War.* No head movement. Exposure 10 seconds.

FIG. 51. Monument visualized as far off and small. Positive. Subject *War.* No head movement. Exposure 7 seconds. One other pair of similar experiments with *War.* gave negative results, i.e. the two photographs were almost precisely the same, neither showing eye-movement.

The experiments using the symphony orchestra and colored print as objects to be visualized gave the following results:

Symphony orchestra.

1. Positive. Subject *Rit.* No head movement. Exposure 15 seconds, determined by experimenter. Report: "I got the instruments. The orchestra was in action, playing rather a lively tune; the tune took different forms. St—— was conducting; I saw a short little fellow with motions corresponding to his. An enthusiastic kettledrum was in action. Violins were carrying the melody. The arms of the violinists were seen moving, principally the first violin." The subject explained further that it was always easier for him to get a visual image if there were auditory images accompanying it. In visualizing the moving train a few days before, he had "had a little of the sound of the moving train." The image of the orchestra was "clearly projected on the screen. The orchestra was brightly lighted." No other photographs were taken of *Rit.* visualizing the orchestra.

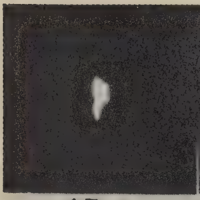
Colored print. (It is not worth while to reproduce the introspective reports.)

2. Positive. Subject *Shil.* No head movement. Exposure 17 seconds, determined by experimenter. No other photographs were taken of *Shil.* visualizing the print.

3. Positive. Subject *War.* No head movement. Exposure 15 seconds, determined by experimenter. No other photographs were taken of *War.* visualizing the print.

4. Positive. Subject *Re.* Exposure 8 seconds determined by experimenter. Head control lacking. One other photograph was taken of *Re.* visualizing the print; it is positive; head control is lacking.

In addition to the experiments in which objects were to be visualized under circumstances in which eye-movement would be necessary in perception, seven were performed in which the subject was asked to visualize the monument or the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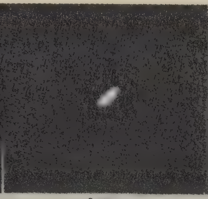
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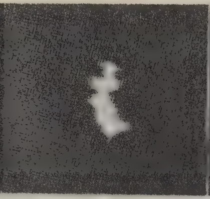
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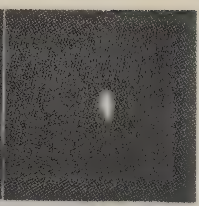
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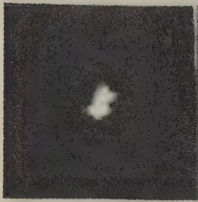
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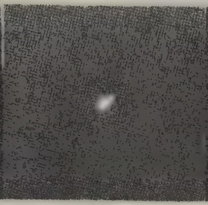
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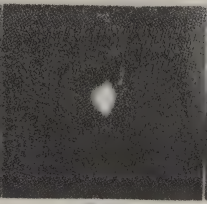
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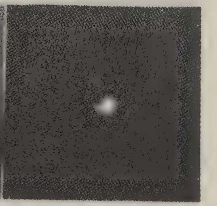
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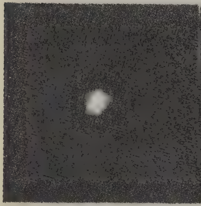
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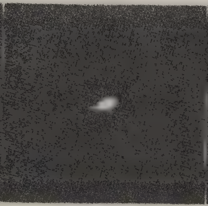
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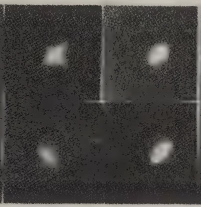
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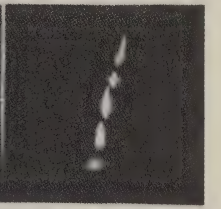
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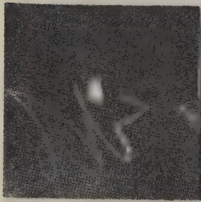
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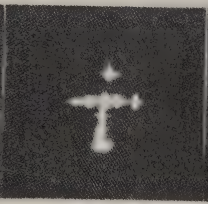
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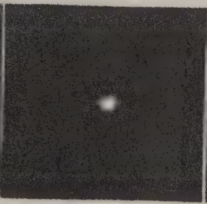
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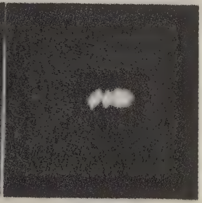
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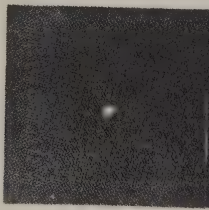
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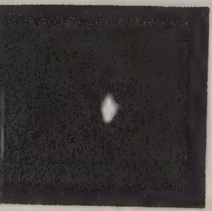
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PLATE VI

cross as if small, or with fixation on a central point, the rest of the object to be seen as if in peripheral vision. Of these, two plates from *Hof.* visualizing the cross and one visualizing the monument, as if with one point fixated, are very clearly "positive" but with the group of fixations very compactly placed as compared with his others. One of the monument seen very small by *K.* the introspection stating that buildings at the side were seen before the image of the monument was secured, shows a sideward shift and no vertical placing of fixations. One other of the monument by *K.*, with central fixation, is "negative," as is also one of the cross by *Hi.* with central fixation. These plates will not be included in the final summary.

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THE INFLUENCE OF CONCOMITANT ACTIVITY AND FATIGUE UPON CERTAIN FORMS OF RECIPROCAL HAND MOVEMENT AND ITS FUNDAMENTAL COMPONENTS

BY
DAVID P. BODER
Lewis Institute

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DAVID P. BODER

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Chicago-Evanston, August 1, 1934.

I. STATEMENT OF THE PROBLEM

In a continuous reciprocal movement such as that of the wrist in semi-supine position we must distinguish with Kronecker, von Kries, and Bryan at least four components; two movements or progressions, i.e., up and down, and two reversals, by which is meant the process of change of direction between the two movements. With the exception of a few early attempts by the first two of the cited authors, the recording of the time of the reversals has never been undertaken, possibly because of technical difficulties.

It is common for the physiologists to analyse pneumographic records from the standpoint of time and pattern of reciprocal action of the muscles of the chest. The same holds for investigations of the characteristics of the kymographic pulse records and electrocardiograms. However, there are very few such analyses of ergographic tracings. Usually only the average number of strokes per unit of time, together with the length of the movement against the weight are obtained and are considered adequate criteria of the performance. Only lately the time between strokes in erograph performance has been considered by Weinland (113). There is, however, one investigation available in which successful attempts are made to analyse quantitatively the kymographic records of reciprocal movement. We refer to the study of Bott, 1923 (19) in which by an ingenious mathematical method the upward and downward progressions of the wrist moving against various loads were analyzed. The procedure involved to some extent a concomitant task since the subject was supposed to control by his action the movement of a pointer within set limits. Bott is definitely aware of the reversal phenomena when he states:

> "Reversal of the stroke phase in reciprocal action involves for one thing, alternation of the innervation of the complex musculature involved, but how and when does this alternation occur in the phase cycle? What do graphic data indicate regarding the nature of the change?" (p. 17)

The answer to the question is deferred in the article, and it seems that the technique found satisfactory for the study of the progressions was inadequate for the analysis of the reversals.

In view of these facts the following problems were formulated:

1. What are the time values for the reversals when the amplitude of movement is kept constant by a device such as a tapping frame?
2. What is the ratio between time used for the movement and the time taken up by the reversal?
3. What bearing has the change of rate of movement upon the time values of the various components, i.e., to what extent is the increase or decrease of duration of one component accompanied by a corresponding change in the others?
4. To what extent is the duration of the components affected by factors known to affect rate, such as a concomitant task, tension induced in another part of the body, or continuous work?
5. What effect has the use of a tool upon the rate of movement, and how does it affect the ratio between the components?

II. HISTORICAL INTRODUCTION

This research deals with a constellation of phenomena which present a common meeting ground for physiology and psychology.

But while for the former they seem to indicate the utmost limit of its sphere of investigation, they present to a great extent the most elementary units from the study of which the psychological interpretation of motor behavior must begin. This seems to be corrobated by the historical development of both sciences. In the seventies of the past century we find physiology to a great extent overburdened with psychological problems, while the psychologists were only beginning to shift from their alliance with philosophy towards a nearer rapproachement with physiology. Coming from the camp of physiology, Wundt gave emphasis to this point of view by calling his fundamental opus a *Physiological Psychology,* although he used the term "physiological" in a rather vague and all-inclusive sense. It seems that the physiologists did not see any difficulty in dealing with the fundamentals of sensory processes, but as soon as they approached the exploration of the effectors, the psychological components of action, and with them the need for a more refined psychological technique, became quite obvious. It is therefore worth noting that in 1879, the year of the foundation of the first psychological laboratory, Kronecker and Hall (65) appear with an article, "Die Willkürliche Muskelaction," published in Du Bois-Reymond's *Archive für Physiologie.* Later the subject is taken up under an almost identical title by von Kries in 1886, a man of equally high standing in both fields, psychology and physiology, but still predominately a physiologist. At the beginning of the nineties tapping experiments are extensively performed by psychologists at leading American laboratories, and the so-called tapping test still occupies an important place in the most diverse studies of conduct, be it Spearmen's (105) investigations of "factors" or the studies on the influence of varied forms of nutrition upon the conduct of normal and pathological subjects, as undertaken by Balken and Maurer (8) at the Chicago city schools and at the Elgin State hospital.

However, the problem has its roots not only in the researches of more or less applied character. Even in its simplest forms, it demonstrates the perfectly legitimate, although sometimes neglected alliance of psychology with philosophy, especially with reference to the cardinal problem of the scope and limitations of voluntary activity. This is possibly one of the reasons that tapping, together with the reaction time experiment, was ceded to the psychologists by the physiologists who could not cope with these problems in view of the great variability of results due to the "human factor"; and in the early experiments both activities were frequently incorporated in one and the same investigation (Dresslar (30), Bliss (16)). We shall not attempt to give an exhausting history of all psychological experimentation related in one way or another to the present research; we shall only try to present the historical high points which must be considered as the direct predecessors of the present investigation.

The material to be reviewed belongs in two groups; (1) the aim and results of the fundamental tapping experiments, and (2) the trend of research referring to concomitant voluntary activity.

1. *The tapping experiments*

We were unable to procure a copy of Kronecker and Hall's (65) article, but the later publication of von Kries, 1886 (64) under a nearly identical heading, gives ample understanding of the former experiment. We learn from this article that Baxt and Yeo, apparently working under Kronecker, were able to record by electrographic methods finger movements in terms of flexion, reversal, and return to the original position. We understand that the task was performed in some kind of a tapping frame within which the subject would operate an electric lever between two adjustable electrical contacts. Von Kries himself used two different methods. One consisted in operating a spring device which would control mechanically a lever at a kymograph, while the other seems to be the first electromagnetic tapping board, since then so extensively used in the psychological laboratories. He studied separately finger movement and hand movement, and found that the amplitude of the movement within certain limits is of no, or but little, significance in determining the rate.

He also emphasizes the complexity of the voluntary movement as compared with apparently similar muscle action evoked by direct electrical stimulation. Referring to the execution of the the shortest movement von Kries in agreement with Kronecker makes the following statement:

> " . . . Vielmehr ist zu erwägen, wie auch Kronecker hervorhebt, dass derartige Bewegungen, sobald man sich bemüht sie möglichst kurz auszuführen, durch das passende Zusammenwirken der beugenden und streckenden Muskeln bewirkt werden. Offenbar setzt die Action der Strecker sehr kurze Zeit nach derjenigen der Beuger ein und schneidet die eingeleitete Beugebewegung ab. Wir erhalten also durch diese kürzesten Bewegungen einen Maassstab nicht für die Thätigkeitsdauer des einzelnen Muskels, sondern fur daskleinste Intervall, in welchem unser Wille zwei Innervationen hervorzubringen im Stande ist, welche getrennt vorgestellt und als successive beabsichtigt werden."[1] (p. 4)

He finds that the shortest time between the beginning of the movement and the initiation of the return amounts to about 61 ms. (milliseconds) which coincides closely with our data. He, however, is unable to give directly the separate values for movement and reversal.

Von Kries distinguishes between successive and repetitive movements: such as in playing of scales on the piano can be performed at intervals of about $\frac{1}{16}$ to $\frac{1}{20}$ of a second while repetitive movements such as in playing octaves are generally not to be expected to be played faster than about 8 per second. It must be remembered that scales on the piano are generally played *legato*, the next finger movement setting in before the return or even reversal of the movement of the preceding finger. In repetitive movements the situation is different: the shortest thinkable interval between two flexions of the same limb is determined at least by the time for extension which must occur between the first flexion and its repetition.

---

[1] "It must be appreciated, as it was also emphasized by Kronecker, that such movements, as soon as attempts are made to achieve utmost brevity, are accomplished by fitting coöperation of both the flexors and the extensors. Apparently the action of the extensors sets in shortly after the action of the flexors, and checks the flexor movement. The time taken up by these shortest movements is not a measure of duration of an activity of a single muscle, but a measure of the smallest interval in which our will is capable to perform two innervations which are thought of as separate and are intended as successive." (p. 4)

Notwithstanding the psychological significance of von Kries' investigation, it was undertaken with predominately physiological aims referring to the problems of frequency and rhythm of innervation. This problem is by no means a closed chapter at the present time and is attacked now by methods more precise than were available at the time of von Kries' study. This aspect of the problem has no direct bearing upon our investigation and we shall turn now to more direct psychological inquiries devoted to the problem.

First of all we find a research by Dresslar (30) done in the Clark psychological laboratory, but under the direction of a physiologist (W. P. Lombard). The purpose is clearly given in the title: "Some influences which affect the rapidity of voluntary movement." The equipment used permitted only the determination of the average number of taps per second. Among the conclusions we find one that may directly interest us: "Increased central activity favors increased rapidity of voluntary movement." A careful study of the experiment indicates that the statement should really read "*Preceding* increased mental activity, etc. . . ." In this connection we find an interesting statement presenting a direct antecedent of all modern studies on concomitant muscular tension. In the author's words:

> "After studying the variations caused by such severe mental work, and taking into account that vigorous physical exercise had produced the opposite effect, it seemed probable that the increase in rate was due to increased central activity and *the unconscious tension of the muscles attending this central excitation*." (p. 523)

However the author seems not to make any distinction between tension and excitement carried over from a preceding task, and similar conditions produced by stimuli directly operative during the performance, as in tapping "after the announcement of a distinguished visitor to inspect the work, and just before reading a paper before the psychological seminary." In a further reference to the decrease in reaction time after intensive mental work (in this case a form of card sorting) Dresslar states:

> " . . . for even if, as has been suggested, the muscles are in a greater state of tension after such mental exercises, it is a result of an unconscious partial innervation of the muscles of the body accompanying increased central activity. If it be true that general muscular tension is produced by mental concentration, it leads to the very important suggestion, viz: that the chief cause of the feeling of bodily weariness resulting from mental concentration, may be due to muscular fatigue." (p. 524)

It is evident that these statements bear a direct relationship to our present day investigations of facilitation or retardation of performance due to muscular tension; and also to the modern studies of progressive relaxation and action current phenomena in the muscles during mental work (Jacobson (60, 60a)).

Following Dresslar we have the extensive work of Bryan, 1892, (23) on "Voluntary motor ability," a direct forerunner of the use of tapping as a test in the fields of applied psychology. Among his conclusions we find:

> "(1) the maximum rate of rhythmical repeated movements is probably a critical test of voluntary control, (2) The maximum rate of movement probably furnishes a test of the general condition of the central nervous rhythm," and (3) "serves as an index of the conditions of the nerve centers by which the muscles involved in the movement are controlled."

More significant appears the following conclusion and the author's comments in connection with it. To quote: "The maximum rate of rhythmically repeated voluntary movement is probably a test of the power of voluntary arrest and reversal." He then continues:

> "It was shown from experiments by von Kries (p. 138), and confirmed (p. 150), that variations in the amplitude of movement within wide limits do not affect the rate. This paradoxical result,—which I have found to hold also for eye movements within the angle of usual move-

ment,—is due to one or both of the following causes. For small distances, and especially for distances less than those usually passed over, the times of arrest, reversal, and of passing through the space nearest the points of reversal, may be slightly increased. In all cases the sum of these times must be so great in proportion to the time occupied in passing through the middle space that a slight increase of the latter is inappreciable." (p. 199)

The quantitative data which corroborate these observations have been found in part by Bott (19) and in part by this research. Bott's findings refer to the decrease in rate in "passing" through the space nearest the points of reversal," while the present investigation gives quantitative information as to the relative length of the time of "arrest and reversal." It must be added, however that the present research has shown that the phenomena observed by Bryan and by Bott are only special cases occurring under certain conditions while under others there is no decrease in rate at "passing through the space nearest the points of reversal" (as observed by direct photography) nor is there in all cases an actual excessive duration of the time of arrest and reversal (see chapter on tool tapping) as compared with the duration of the movement. About the same time and during the years following a vast number of tapping experiments were undertaken under most varied conditions and for an infinite number of psychological aims. Some of them will be mentioned in the further discussion. The studies from the Yale psychological laboratories appear especially significant. The present day status of the inquiry as to its implications for general psychological problems has not much advanced from the situation best pictured in a series of articles by Wells, 1908–1909 (114, 115, 116, 117).

The first article, under the title "*A neglected measure of fatigue,*" gives an extensive review of previous tapping experiments, together with a discussion of the fatigue problem. The second article brings the results of his own research on normal tapping. From the large number of conclusions we shall quote only those of immediate interest: (a) "Initial rate and fatigability are negatively correlated, a fast initial rate being usually accompanied by high fatigue loss." (b) "Fatigue tends in all respects to decrease variability; no consistent effect on variability due specifically to warming up or practice can be traced."

The third and fourth articles by Wells are devoted to studies involving pathological and sex differences respectively. The further investigations up to date continue to use one or another forms of tapping, either in the studies of special mental states such as those involved in the problem of drugs, neuroses, etc., or as parts of the various batteries of mental and especially motor tests.

It seems pertinent to mention the findings of Gates and Taylor (53) who studied "the nature of improvement resulting from practice in a motor function" by means of tapping. They found that in children between 5 to 8 years of age, maturity is a predominant factor over practice. They infer that,

> " . . . long continued training motivated as effectively as practicable, did not . . . improve the neural machinery in any direct manner. . . . The group which practiced 111 days showed no superiority at the end to the group which practiced only 18 days at the beginning and 17 days at the end of the period." (p. 235)

In conclusion we must state that in spite of its long standing there has been no satisfactory standardization of procedure of the tapping test and no refinement in recording of the results in any more analytical manner than strokes per time unit or time consumed per stroke.

2. The trend of research referring to concomitant voluntary activity

The second problem attacked in this research is that of concomitant activity. In older experiments it is identified with the problem of attention span. A decrease in efficiency of one activity or total break-down during a concomitant response, was attributed to the inability to lend to both activities the necessary amount of attention. One of the early experiments is that of Swift, 1892 (107).

> "The purpose of these experiments was to find how the simple 'muscular' and the 'choice' reactions would vary while the reactor's attention was directed to certain kinds of work. Three tasks were

> given; (1) repeating a poem already committed to memory; (2) reading an English book; and (3) reading Kant's *Kritik der Reinen Vernunft.* The time for the simple muscular and choice reactions under these conditions was learned." (p. 17)

The results indicate a significant increase in reaction time when the reaction experiment is superimposed upon the process of reading.

Another investigation directly referring to the problem of concomitant response during tapping is that of Bliss, 1893 (16) performed at the Yale laboratories. He gave to his subjects the task of adding various numbers, such as 214 and 23, with the following results: The concomitant task

> " . . . at first made the rate very regular, more so than at any other point of the series. This was during one period between hearing the problem and beginning to solve. . . . The first attempt at solution produced marked irregularity and real work was accompanied by the steady falling off in rate. As soon as the answer was reached the attention returned to the tapping and the rate rapidly recovered." (p. 5)

Bliss postulates more than one form of attention, and among them "subconscious attention." He explains the obtained results in the following manner:

> "This sudden increase in the regularity of tapping without a marked change in the rate, when the attention is attracted by some other object, is similar to the fact noticed in some of the experiments in reaction-time, namely, that more regular results were sometimes obtained when the mind was partially absorbed in other things. The more superficial, ideational attention is directed to them while the unconscious muscular attention which is largely a matter of habit runs along more smoothly and automatically. As the mind is absorbed in the secondary problem, the more substantial subconscious attention gradually withdraws from the muscular effort and reinforces the mental effort. . . . "
>
> "In the case of the change first described, namely the variation from tap to tap, the cause is not so evident. The fact that a partial withdrawal of the attention stops it and makes the intervals regular indicates a psychical cause. Under the additional strain of conscious voluntary attention the nerve mechanism acts more irregularly. Irregularity seems to be a characteristic of the higher forms of psychic life.

The usual explanation is that there are two sets of nerve centres involved, the higher more unstable brain-centres and the lower more automatic ones of the smaller brain and spinal cord. A disturbance of the attention is supposed to cut off the higher centers from the circuit engaged in the muscular action. Yet both in the tapping and the reacting it was seen that further central activity was accompanied by further decline in the muscular rate." (p. 51 and 52)

We see clearly from these experiments that psychology has carried over from every day life not only the current notions of attention and distraction, but has adopted the terminology which appears to imply some form of special displacement of neural activity from one region of the brain to another. The present day controversy of the nature of inhibition is rather clearly indicated by Bliss's terminology of "withdrawal" and "cut off."

In connection with this point one is tempted to quote somewhat at length from a research done by Hugo Eckener (33), the present commander of the Graf Zeppelin, while working under Wundt, 1893. Referring to the fluctuation in the ability to perceive minimal sensory stimuli and attributing it to the inability to maintain continuously a memory image of the stimulus in consciousness he makes the following statement:

"Wenn, wie die Selbstbeobachtung jederzeit erweist, eine jede psychophysische Erregung des Centralorgans eine Hemmung fur das Eintreten einer anderen bedeutet, und wenn eine aussere Reizung, um eine Empfindung auszulösen, desto stärker sein muss, je verschiedenartiger von ihr und stärker der augenblicklishe hemmende psychophysische Process im Gehirne ist, so muss offenbar ein äusserst geringer andersartiger, von Bewusstsein begleiteter Reizungszustand des Centralorganes schon im Stande sein, das Bewusstwerden einer minimalen Reizung zu verhindern."[2] (p. 386)

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[2] "If each psycho-physical excitation of the central nervous system has the significance of an inhibition for the appearance of another excitation (as corroborated by introspection), and if an external stimulus, in order to evoke a sensation, must be the stronger the greater the difference in modality and strength of the inhibitory process active at a given moment, then a rather weak stimulus but of different modality which has reached consciousness will be able to prevent the appearance into consciousness of a minimal stimulus." (p. 386)

We return now to our fundamental problem. The definition of a concomitant task presents some minor difficulties. It appears to depend on the experimental situation. Generally the responses which are more extensively investigated are considered belonging to the fundamental task, while those treated with less control and precision would appear as the concomitant activity. So in Swift's work we have reading of texts of various degrees of difficulty as a concomitant task to reaction time. There is in general some difficulty in establishing which task is, so to speak, the fundamental, and which the concomitant. The investigators generally do not take equally efficient records on both performances. The painstaking work of Misbach, 1933 (79, 80) presents in this respect one of the rare exceptions. In general much depends upon the subject's attitude and as Swift has already observed: "... there are no means by which the amount of attention given to the reaction or to the task can be measured." (p. 17)

We shall turn now to more recent experiments. It seems that in our inquiry we must distinguish, at least on the technical side of experimentation, between complex activity and multiple activity (Mehrfachhandlung).

The activity in a choice reaction can be considered to be more complex than in a simple reaction; the response to a stimulus word consisting of a synonym is more complex than if the response requires "the first word that comes to the mind." It seems that complexity of action can be characterized by the amount of precision which often implies a greater amount of inhibition. The necessity to respond in a determined way becomes "more complex" with the degree of specificity of the response. This is often achieved only when we have mastered the supression of other more spontaneous responses usually given in a similar condition. Ach's experiments are almost entirely of this "complex" category.

In the same class although possibly in a special category must be placed choice reactions involving simultaneous response with more than one limb, although some investigators refer to them also as multiple activities (Mehrfachhandlungen). We have in mind the recent work of Mohnkopf, 1933 (81). This investigator presented his subjects with a long series of choice reactions involving 15 different stimuli (presented one at a time) and correspondingly 15 different responses. A number of these responses had to be performed by means of two or more muscular groups, such as raising both feet, or pressing certain devices with the toes of one foot and the heel of the other, or even with the heels of both feet and the toes of one (p. 240). The Author calls these responses "multiple action" (Mehrfachhandlung). This appears to be rather an unfortunate connotation since he has to introduce the term "double action" (Doppelleistung) for responses termed by others as multiple actions. We should prefer to reserve the term multiple activity for responses which involve the simultaneous performance of two or more different tasks in response to two or more different stimuli.

Such experiments were performed by Mohnkopf, whose subjects had to react by movement of one or both lower extremities to a tactual stimulus and by a hand movement to a simultaneous visual stimulus. We shall return to the discussion of Mohnkopf's findings after a review of some earlier works.

We have already discussed the experiments by Swift and by Bliss. Both gave the subject some concomitant tasks. The former gave a reaction time experiment during the recitation of a poem or reading of material presumably of various degrees of difficulty, the latter gave mental arithmetic during tapping.

The upswing of industrial psychology during and after the World War is marked by the production of a number of tests on span and multiplicity of attention. As would be expected in the procedure of test construction the authors were mostly interested in the correlations of the test performance with actual efficiency and with other test scores. There has been but little evaluation of the findings with reference to general psychological theory.

However, in the last few years a number of valuable contributions have been made. Moore and Gilliland, 1921, (82) have used voluntary control with concomitant addition of numbers as an item in personality testing. Fearing, 1925, (37) has investigated the voluntary control of sway concomitantly with counting of clicks given at irregular intervals (vide ut infra). Luria (68) has shown that the pressure upon a key during neutral and emotionally toned associations is not only different in its time characteristics but in pattern and intensity as well.

In an extensive investigation of multiple action Schorn, 1928, (100) performed the following experiments: (1) "Double addition." The subject was to perform the written additions in one of the tests of continuous work. Every three seconds two one-place numbers were called out by the experimenter and the subject had to give orally the sum of the two, while the original task was continued. (2) A simple word association test was given orally while addition in writing was performed. (3) During the process of addition as above the subjects had to listen to a story, which had to be reported afterwards in written form. Experiments 4 and 5 refer to the problem of testimony. The subjects were quizzed on a trivial occurrence between a pupil and the teacher which took place while the whole group was taking one of the arithmetic exercises as above. In the other case again a story was read with instructions to listen closely in order to report afterward, while reading simultaneously a number of passages (also for further recall) which contained somewhat related material to the story.

The author finds: (1) Some consistency in the subjects' attitude in various experiments especially in tasks of lesser difficulty. (2) Simple concomitant tasks have no influence upon the fundamental activity and may produce even some improvement in the majority of subjects. (3) The report of the stories was less efficient when work was done during listening to them; and work on related material has a more detrimental effect than unrelated.

The next investigation to be analyzed is that of Kreipe, 1930, (63) under the title: "Über die Mehrfachhandlung und über das Gesetz der speziellen Determination." The greater part of his experiments belong to the category which we have designated as a special category of complex responses as distinguished from multiple responses. His subjects are instructed to react to a certain letter with one foot and one hand; to others with both feet; to others with one hand and both feet, etc.

The reaction is certainly complex both on the side of the stimulus and on the side of the response, but the situation must be differentiated from the multiple type of reaction as given by the same author. In the latter, nonsense syllables were presented one by one at a rate only sufficient to be read, and the subject was instructed to name the middle letter. Concomitant to this first situation another was introduced. The subject's hands and feet were resting upon reaction keys, and he was instructed to watch the color in which the syllables were printed. Red required a reaction by raising the right hand, and the three other colors had a similar significance for the other limbs. A number of syllables were printed in black and did not require any additional reaction to the naming of the middle letter. The results of this particular experiment are rather significant. In none of the 104 cases in which a simultaneous response was required was this response correct. In 73 instances the subject missed the middle letter of the next syllable. The subjects reported that the perception of stimuli was simultaneous, but the simultaneous execution of both responses was impossible. It appears that the difficulty lies in the arrangement of the experiment. First of all it can not be expected that discrimination of a letter, and discrimination of a color will give the same reaction time; second, it is not to be expected to obtain the same reaction time when reacting by voice or by a hand movement; third, an identity in reaction time cannot be expected when a response involving four alternatives is paired with a perceptual situation (recognition and naming of a letter) of rather different character. The experiment however suggests that the principal difficulty in concomitant activities would lie not so much in the simultaneous execution of two responses (to two different stimuli) but in the simultaneous initiation of the same.

We return now to Mohnkopf's work (81). Like Kreipe he is interested in further elaboration of evidence for Ach's (1) "principle of special determination," and we shall refer to this topic in the chapter devoted to theoretical discussion of our investigation.

The title of his monograph "Zur Automatisierung willkürlicher Bewegungen" indicates that he is predominately interested in the process by which an originally conscious, voluntary act becomes "automatized." We must state that psychological writings for about the last twenty years use the connotation of automatisms only in the sense of Ach's "automatisierte Willenshandlungen" (automatized voluntary action) (1). The physiologist still continues to use the term for such functions as breathing or heart beat and one has to be on the alert in qualifying his terminology. (Fearing (42), also Ziehen (122), see footnote in the 1924 edition.) Considering that the fundamental characteristic of automatization is *the reduction of conscious control to a minimum so that other acts can be consciously attended to*, Mohnkopf in accordance with Ach suggests that the ability to perform a concomitant task can be used as a criterion of automatization.

His apparatus (devised by Pauli (91)) and method of response are, with some modification, the same as Kreipe's (see p. 16). The stimulus was a tactual one. We shall again make reference only to the part of the investigation concerned with concomitant activity defined as the simultaneous occurrence (at least in part) of two or more different responses to two materially different stimuli. The concomitant reaction was elicted by means of the following arrangement. A visual stimulus consisting of a card with various figures was presented in a stereoscope 36 ms. before a tactual stimulus. The presentation of the visual stimulus earlier than the tactual one was based on previous investigations by Schulze (101) who found that under such conditions the experience of simultaneity is, according to introspective reports, most perfectly achieved. The response to the visual stimulus consisted in noticing and reporting of the position of two similar figures arranged with two dissimilar ones in various spacial relations. The original author—Grünbaum (58)—used them in a study of abstraction. From the description it seems apparent that the report on the visual stimulus was not necessarily to be given simultaneously with the release of the reaction key in response to the tactual stimulus. It was, so to speak, an *Aussage* experiment with distraction, the latter involving another stimulus and the act of response to the same. It is evident that only the stimuli were relatively coincident while the responses could be successive.

The findings coincide to a great extent with those of Kreipe as to improvement through practice and as to corroboration of Ach's principle of "special determination."

It appears from the literature that the most systematic experimental attack upon the problem of simultaneous activity is that of Misbach (79, 80) who to some extent has followed up a procedure established by Fearing (38). Misbach's work contains not only the detailed data for the knee-jerk but also for the responses in a concomitant reaction time experiment; so that the reciprocal influences of both situations can be well analyzed.

The problem of concomitant activity, with special reference to the motor component has also been attacked by means of the study of changes in handwriting due to superposition of an additional task. It must be stated that handwriting is predominately reciprocal muscular action involving the progressions up and down and the bottom and top reversal. It is evident that if only a proper method of reliable analysis could be devised, we would have in handwriting an abundant material of study of motor phenomena. The literature on the subject is rather extensive and we shall limit ourselves to one research published during the past year.

Stracke, 1933 (106) has investigated the influence upon handwriting, (1) of an additional mental task, (2) of an additional manual task (pressing a reaction key with the left hand), (3) of forced increase of writing speed. A number of factors were used as criteria of change, such as amplitude of movement, pressure, size of angle between the longer letters and the base line, etc. The results appear to be positive and indicate improvement due to practice (automatization of performance).

In toto, the reviewed literature indicates that the great majority of cases were devoted to combinations of reaction time procedures with concomitant mental tasks or the combination of one mental task with another. Fearing (38), Dodge (27a), and Misbach (79) worked on the juxtaposition of two motor responses, one of them being a reflex (the knee-jerk). There are hardly any experiments on the juxtaposition of two voluntary motor responses except those described in the handwriting studies, with their rather cumbersome technique in interpretation of results.

The early experiments of Swift and Bliss, both giving mental operations concomitant to tapping have not been followed up at least in a systematic manner.

The ergograph experiments on work performed with both hands, do not imply either difference in action or difference in stimulation for each hand movement. Reserving for concomitant activity the experimental conditions which imply at least two different stimuli with two respective responses, we can present a table of various combinations of two which would make up a program of investigation in this field:

1. Two mental tasks (listening to one story while reading another)
2. A voluntary motor response and a mental task (tapping, controlling sway, or staring while analyzing pictures or solving problems)
3. Two reflexes (knee-jerk and pupillary reflex)
4. A reflex and a mental task (knee-jerk with an attentional task)
5. A reflex and a voluntary motor response (knee-jerk and hand or eye-lid movement)
6. A voluntary movement with concomitant voluntarily induced tension (tapping with one hand while squeezing a dynamometer with the other)
7. Two different voluntary movements (pressing a reaction key in response to sound while writing, provided it is the character of handwriting performance we are interested in, or operating a lever in response to a specific stimulus while tapping as done in the present experiment).

Following the original formulation of our problem, we have chosen conditions 2 and 7 for our research. For this purpose we have investigated the influence of concomitant activity upon the total rate and upon the duration of the components of reciprocal hand movement. In the first series the concomitant task involved attention to a sequence of visual stimuli with subsequent verbal report; the second series required a number of concomitant choice reactions involving an intensive motor component. The rest of the investigation as stated before is devoted to the study of the influence of the use of a tool, and of fatigue and recuperation upon the same variables of reciprocal hand movement. In addition some data referring to condition 6 have indirectly been obtained.

III. THE APPARATUS

The subject was seated in an ordinary chair before a table on the edge of which was fastened a small frame of 4 cm. inner vertical clearing and about 8 cm. in the horizontal dimension. The horizontal stops were made of 0.3 inch iron rod fastened to insulating material. The height of the table was so adjusted that the lower bar of the tapping frame was thirty inches from the floor. A two element oscillograph Gen. Electr. D12 was used. A 60-cycle commercial current was led through a toy transformer to the power galvanometer to give a time line of 60 units per second. This was found indispensable in order to simplify the counting of time units greater than 17 ms. The other galvanometer was in series with a more complicated circuit, since the intention was not only to compare the reversal time with the progression time, but the top reversal time with the bottom reversal and each of those with either the upward or downward progression. For this purpose a circuit was designed, involving only a single '27 tube. The subject's body was put in circuit with the apparatus, by fastening a lead from the grid with a small metal surface to the palm of the left hand by means of adhesive tape. It was advisable that the index finger of the right hand used for tapping should be dipped in plain water, to make the contact more definite[3] (2). (The diagram of the circuit is given on page 24; and here it is sufficient to say that by touching the upper bar of the frame the subject would make the current more positive and draw the galvanometer beam upward, by touching the lower bar he would give the current a "negative bias" and draw the beam downward). During the progression the galvanometer was on the "neutral," and depending upon whether this neutral stretch was preceded by a top or bottom reversal it was interpreted respectively as up or down. In order not to depend upon indirect measurements for fractions less than one cycle or 16.6 ms. the response circuit was led through a tuning fork oscillator of a frequency of 500 d.v. per second. This frequency was checked for every experiment directly from the photographic records.

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[3] Some subjects would detect a slight shock; they were always urged to explore the contacts well, and if a shock was present it was eliminated by a corresponding reduction of the current.

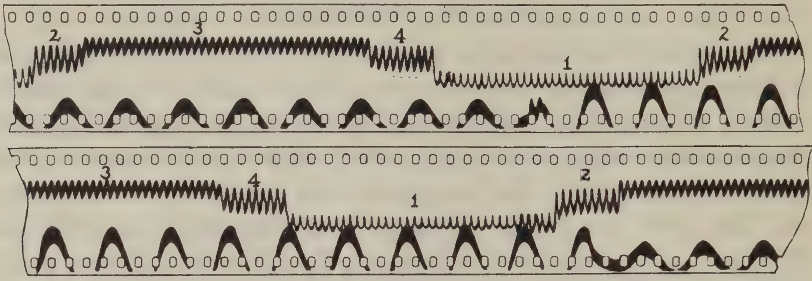

Fig. I. Oscillographic Record of Finger Tapping

Upper Wave: 500 cycles per second. (*1*, Bottom Reversal; *2*, Progression Upward; *3*, Top Reversal; *4*, Progression Downward.)
Lower Wave: 60 cycles per second. (Change of height of this wave indicates appearence of concomitant stimuli.)

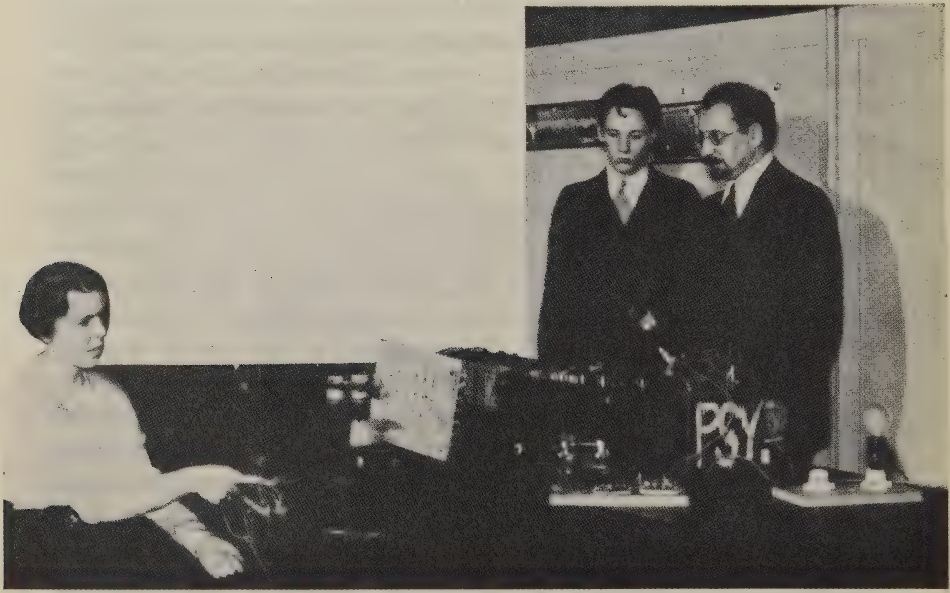

Fig. II. General Arrangement of the Experiment

A few words should be said about the photgraphing technique. The oscillograph used comes with a camera that hardly holds more than about 10 feet of film or bromide paper; as a matter of fact it is calculated for a $2\frac{1}{4}$ by $3\frac{1}{4}$ film. With the coöperation of the General Electric X-Ray Corp., a camera for recording purposes was constructed to which a 200-foot motion picture magazine was adjusted. By means of a gramophone motor, properly geared,

BOTTOM	UP	TOP	DOWN	STROKE
73	19	48	17	157
69	12	55	17	153
75	21	44	17	157
65	14	62	17	158
69	17	67	12	165
62	17	69	12	160
67	14	65	14	160
71	12	54	14	151
73	14	53	17	157
75	12	58	14	159
73	19	55	12	159
69	12	75	17	173
75	17	64	12	168
71	10	75	14	170
71	19	59	14	163
64	12	69	17	162
71	14	64	12	161
71	12	62	14	159
67	12	62	14	155
71	12	61	12	156
67	17	60	14	158
65	12	67	14	158
64	12	61	14	157
64	8	75	14 (end)	161
Av. = 69.3	Av. = 14.2	Av. = 61.8 Stroke = 159.7	Av. = 14.4	Av. = 159.9
43%	9%	39%	9%	

FIG. 3. ORIGINAL RECORD OF READINGS OBTAINED FROM A FINGER TAPPING EXPERIMENT LASTING ABOUT 4 SECONDS

One subject. Time in milliseconds

Frequency 500. ms./cy. 2.0. S. Galazan. April 16, 1934. Finger tapping, motor noise. No. 277.

the film could be driven at a rate of from 3 to 4 feet per second. About 100 feet of film could be taken before the camera would begin to lag. Therefore, it was advisable to remove the film from the take-up section after every ten experiments, counting about 6 to 10 feet per experiment, which generally comprised one experimental period for one subject.

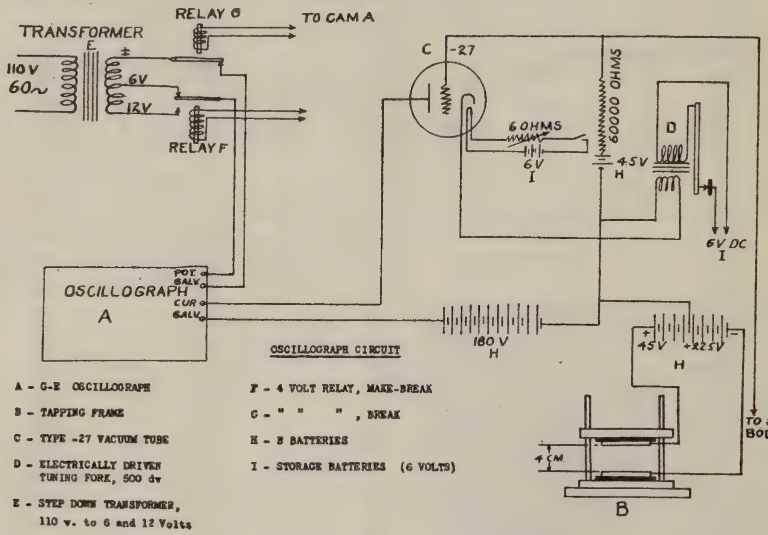

FIG. IV. OSCILLOGRAPH CIRCUIT FOR THE EXPERIMENT

IV. NORMAL FREE HAND TAPPING AND THE INFLUENCE OF A CONCOMITANT IDEATIONAL TASK UPON THE PERFORMANCE

1. *Purpose.* (*a*) To investigate the rate of normal tapping under our experimental conditions, as described in the instructions below, and to establish the time required for each of the four components of the stroke, i.e., bottom reversal, upward progression, top reversal, and downward progression. (*b*) To ascertain to what extent the rate of the total movement or of any of the components might be affected by a concomitant task of the testimony type (Aussage), in which the subject has to watch for a sequence of visual patterns appearing in rapid succession, and to report after presentation of entire series.

We have called the described concomitant response an ideational one in order to differentiate it from a predominantly motor response. By ideational we mean a form of behavior which is predominantly cognitive, involving but little motor components other than speech or symbolic gestures.

We consider that the different instructions do create different mental attitudes, which we have designated as mental set.

All tapping performed without a tool we have designated as finger tapping.

All subjects were male college students, and only those writing with the right hand and considering themselves otherwise right-handed, were used in this research

2. Apparatus and procedure

(*a*) *Normal finger tapping.* The subject was seated on an ordinary chair before the table on which the tapping frame was adjusted as described on page 21. After the lead from the grid of the amplifier tube was fastened to the left hand the instructions were read as follows:

> "This is a study of speed of muscular movement. We are interested to see how fast you can move your index finger within this frame, at full width, touching each time this bar (pointing to the lower bar), and this bar (pointing to the upper bar). Close your fist except for the index finger, which you are to keep stretched, and keep this position of your hand throughout so that the palm be directed inward and be always perpendicular to the surface of the floor. Do you understand? Try it. Ready, go." (Subject would tap for a few seconds, while the experimenter would ascertain whether he followed the instruction.) "Stop. That's fine."

If subject would report a slight electric shock the potentiometer would be adjusted accordingly. Occasionally the surface of contact of the lead to the left hand would have to be reduced. Then the fundamental instructions were repeated, the illumination in the room reduced, and the camera placed upon the oscillograph. A middle size motor with an interruptor device attached (such as are used for busy signals at small telephone exchanges), was then set in motion. This produced a great deal of noise, but of a constant character. This was done for the following purposes: (1) the motor was needed for some of the later experiments and therefore it was turned on during all of the experiments, (2) the loud noise would mask the hum of the motor of the camera, thus making it impossible for the subject to know when the photographic record was actually being taken. After two or three practice trials, each lasting about five seconds, and between which the subject was instructed regarding the importance of speed, again a warning signal of *Ready*, followed by *Go* was given and about a second later the camera was started and run for about 2 to 3 seconds. Once the camera was stopped the order of *Stop* was given to the subject. Such an experiment would yield from 15 to 25 or more countable strokes, each stroke composed of the four variables: Bottom reversal, Upward progression, Top reversal, and Downward progression. The films were developed, the strokes counted, and records made according to the sample. (See Fig. III.) For results, see table I under *Normal*, and the discussion at the end of the chapter.

(*b*) *The attention-memory experiment.* The term memory is introduced because the subject had not only to watch for certain signals but to report later their pattern and sequence of appearance. The subject was instructed to attend, to remember, and to report afterwards (a form of Aussage experiment). The additional apparatus contained two separate elements, as follows: (1) on a board 9″ by 9″ were fixed 18 flash light bulbs, of 3.2 volts, six lights in a row; two of them (number 10 and number 18) were connected with a flicker device and were flashing at a rate of about 12 times per second. This served as a background so to speak for the sequence of stimuli to be discriminated and to be remembered.

(2) By a special wiring system involving a stepper switch, such as used in the automatic telephone system, groups of lamps from among the remaining sixteen (one to five in a group) were flashed on; each flash lasting about one-fourth of a second and with an one-fourth of a second interval between flashes.

The procedure was as follows: The subject was given detailed instructions and the light groups flashed slowly by a hand switch one after another, and the subject given enough time to identify their number (without simultaneous tapping). After it was ascertained that the instructions were understood, the subject would be given two or three trials at regular rate, while tapping (the subject being unaware that no photographic records were made). Then the combination controlling the sequence of flashes was changed and again a practice trial was given.

At the conclusion of these preliminary trials the following four experiments were undertaken:

Experiment A: Attention-memory experiment.

Instruction: "Now watch the lights carefully. Ready, Go."

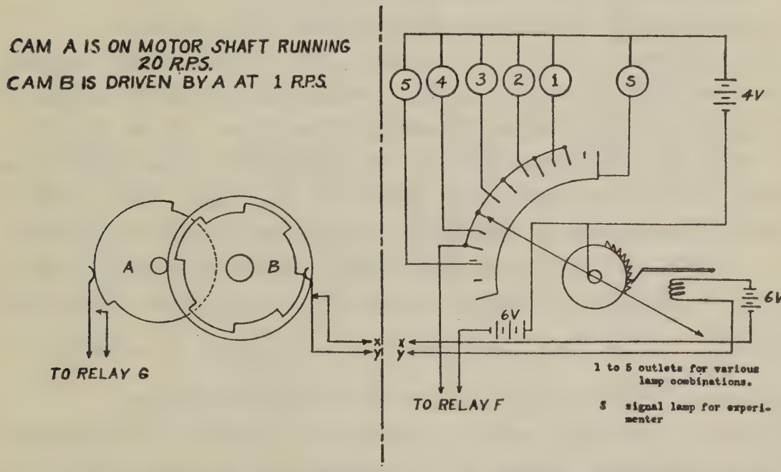
CAM A IS ON MOTOR SHAFT RUNNING 20 R.P.S.
CAM B IS DRIVEN BY A AT 1 R.P.S.
A
B
TO RELAY G
x
y
5
4
3
2
1
5
4V
6V
6V
x
y
TO RELAY F
1 to 5 outlets for various lamp combinations.
S signal lamp for experimenter

Fig. V. Circuit for the Presentation of Successive Visual Stimuli

After about a second of tapping the camera was released and immediately afterwards the stepper switch, which, as stated before, flashed automatically the five stimuli at intervals of one-fourth of a second, was put in action. A light signal at the experimenter's desk would indicate the time to stop the camera and to order: "Stop. Report." The reports were on the average about 40 to 60 percent correct; i.e., rarely more than three light groups were correctly reported as to number and place in the series.

Experiment B: The deception experiment.

The flickering lights would continue in action. Subject was ordered, "Let's have it again. Watch for the lights and the order of their appearance, and report afterwards the number of lights in each group, and the order in which the groups appeared. Ready, Go." But the lights would not appear. After about two seconds of photographing, experimenter would order "Stop" and ask for the report. When the somewhat baffled subject would state that there were no lights, experimenter would apologetically explain that he apparently has forgotten to throw the proper switch.

Experiment C: Attention-memory experiment repeated.

"Let us have another one. Watch carefully for the lights."

Conditions and procedure same as in A.

Experiment D: Deception with negative instructions.

Experimenter says: "Let us have just a plain finger tapping. Don't pay any attention to the board, there will be no lights. Ready, Go." But the lights would appear just as in A and C. The majority of the subjects reported that they had noticed the lights, but did not pay any attention to them One tried to report the sequence of the lights; one stated he did not see any. The results are tabulated on table I, page 29, while table II gives the absolute difference between the compared values and the $\frac{\text{Diff.}}{\sigma\ \text{diff.}}$ which has been used as a criterion of reliability of difference (or similarity) between the compared values.

Results. Tables Ia and IIa present the results of the first part of this experiment. They give the records for ten subjects each of which was given the task once under each of the five conditions. "Norm." means finger tapping before the attentional task has ever been discussed. In experiments A and C, the subject has to attend to the appearance of the various groups of lights with the purpose of reporting their number and sequence. In B, the instructions were the same as in A and C, but the lights did not appear, as if by mistake; and in D, the instructions were: only to tap and that no lights will appear, but as if by mistake, the lights did appear. The results for the five series have very much in common.

(1) The time values for bottom and top are between three and

TABLE IA

Attention memory with deception

Number of subjects 10

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
Norm.	62.8	9.3	14.8	237	17.3	4.7	27.0	235	64.8	13.1	20.2	234	16.6	3.0	18.1	234	161.0	20.2	12.5	237
A	63.4	12.0	18.9	225	19.2	7.7	40.3	224	72.4	16.2	22.3	226	18.3	5.5	29.8	224	171.9	20.4	11.9	224
B	62.6	9.5	15.2	173	18.8	6.1	32.4	173	69.7	13.6	19.6	173	18.6	4.7	25.5	172	168.3	17.1	10.2	173
C	65.2	7.7	11.8	218	17.6	5.6	32.0	221	71.8	16.4	22.9	220	17.3	4.9	28.6	219	169.9	20.0	11.8	221
D	65.0	7.8	12.0	211	16.1	5.3	32.8	215	65.6	10.1	15.5	214	15.6	4.0	25.8	214	160.5	17.2	10.4	215

TABLE IB

Attention memory without deception

Number of subjects 5

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
Norm.	58.9	13.4	22.8	145	24.7	5.5	22.1	145	59.7	15.2	25.5	147	19.6	4.9	24.9	145	161.9	21.9	13.5	147
A	65.7	13.7	20.9	138	22.4	6.6	29.3	140	60.5	13.9	23.0	138	18.9	5.5	28.9	140	166.1	25.4	15.3	139
C	66.3	19.8	29.9	129	20.9	5.1	24.3	129	63.9	14.1	22.0	127	18.8	3.9	20.6	128	167.1	27.5	16.4	129

Summary of tables Ia and Ib

Total number of subjects	15
Total number of strokes	1,485
Total number of photographic readings	5,913

TABLES IIA AND IIB

Tabulation of differences between values obtained under various experimental conditions in terms of absolute differences in milliseconds and of the indices of relative reliability of these differences

TABLE IIA (10 SUBJECTS)								TABLE IIB (5 SUBJECTS)			
Conditions compared	Instructions (attentional task)	Technique	Memory factor	Stroke		Top reversal		Stroke		Bottom reversal	
				Diff.	$\frac{\text{Diff.}}{\sigma \text{ diff.}}$	Diff.	$\frac{\text{Diff.}}{\sigma \text{ diff.}}$	Diff.	$\frac{\text{Diff.}}{\sigma \text{ diff.}}$	Diff.	$\frac{\text{Diff.}}{\sigma \text{ diff.}}$
				ms.		*ms.*		*ms.*		*ms.*	
Norm. vs. D	Same	Same	Same (absent)	0.5	0.3	0.8	0.7				
B vs. C	Same	Different	Different	1.6	0.7	2.1	1.8				
A vs. C	Same	Same	Same (present)	2.0	1.0	0.6	0.4	1.0	0.3	0.6	0.3
A vs. B	Same	Different	Different	3.6	1.9	2.7	1.8				
Norm. vs. B	Different	Same	Same (absent)	7.3	4.0	4.9	3.7				
B vs. D	Different	Different	Same (absent)	7.8	4.6	4.1	3.3				
Norm. vs. C	Different	Different	Different	8.9	4.7	7.0	5.0	5.2	1.7	7.4	3.6
C vs. D	Different	Same	Different	9.4	5.3	6.2	4.8				
Norm. vs. A	Different	Different	Different	10.9	5.8	7.6	5.5	4.2	1.5	6.8	4.2
A vs. D	Different	Same	Different	11.4	6.4	6.8	5.2				

Explanatory notes:

Norm.—Tapping without superimposed ideational task.

A and C—tapping under instruction to watch the groups of appearing lights and to report after signal "stop."

B—deception experiment: Instructions to watch the lights, but lights do not appear.

D—deception experiment: Experimenter informs subject that lights will not appear, but they do appear.

Table IIb refers to the control experiment (see table Ib) in which B and D were omitted.

four times as long as the progression times. It is evident that the reversal time or the time of stops at the barriers absorbs, under such conditions, about three-quarters of the total time, while the procedure is intended, and *introspectively perceived, as incessant motion.*

(2) The average time per stroke in the five experiments fluctuates in round numbers only from 160 to 170 milliseconds in spite of some gross variations in conditions of mental set and technique.

(3) Inspection of table Ia shows that in four of the five columns the data could be arranged in two groups: A, B, C on the one hand, the Normal and D on the other. This is especially evident for the *stroke* and for the *top* reversal. Since the smallest photographic unit actually recorded and counted corresponds to 2 ms., it seems advisable not to rely upon differences of 3 ms. or less, except as possibly expressing a tendency. An analysis of the column "stroke" then indicates a near identity for the values of *Norm.* and of *D.*, and a great similarity between A and B and C.

The same holds for the column "top." As stated before, A and C were identical in instruction and technique, B was identical in instruction with A and C, but different from them in technique; as a matter of fact the technique of the experiment B was identical with normal but the instructions were different. The results for stroke and top reversal indicate that the similarity in *mental set* or mental attitude seems to predominate over similarity in actual stimulus conditions. This is corroborated by the values for D and "normal." In D the technique was identical with A and C, but the meaning of the instructions is closer to those of the *normal* series since in the latter there were no instructions referring to lights, and in D the instructions were "not to expect any lights." Again the time of the response is more influenced by the mental set than by the actual arrangement of the stimulus pattern. However, the conclusions as to the specific elements of the stroke affected by the concomitant task have to be drawn with caution, since in a second experiment the results were not entirely identical in this particular respect.

Interested in the influence of the Deception experiment upon the subsequent "non-Deception" trial C, we decided to give only

Norm., A, and C to another five subjects. The results are given in tables Ib, and IIb. Again inspection indicates that there is greater similarity between A and C than between either of them and the *normal.* However, here it is no longer the top reversal and stroke which are numerically most affected, but the bottom reversal alone. The strokes tend to be somewhat longer, but the differences are not reliable. However, the bottom reversals in A and C show marked similarity among themselves and a reliable difference when either of these values is compared with the corresponding value in the normal series. There appears nothing in the record that would suggest an explanation for the question, why with one group the top reversal should be affected, while with the other the change appears in the bottom reversal. The number of subjects in the second group was only five and at least in part the phenomenon can be attributed to marked individual differences. Otherwise, the second series presents the same form of relationships between reversals and progressions, the former being two to three times as large as the latter. The averages for the time per stroke range from 161.9 to 167.1 and fall within the range of averages for part one where they fluctuate between 160.5 and 171.9. It is evident that, whatever effect a mental set in this experiment has upon the tapping under conditions described, it affects but slightly the rate of tapping as such. The effect is more pronounced upon separate components of the process, predominantly upon the reversal time. This in turn may either lead to a slight increase in the time per stroke or be compensated for by a corresponding decrease in the time of the other components. The analysis of variability of performance does not offer any consistency in relationship to the superimposed ideational task as defined above. However it presents some interesting features in other respects which will be discussed in a separate chapter.

V. THE INFLUENCE OF A CONCOMITANT ATTENTIONAL TASK WITH AN INTENSIVE MOTOR COMPONENT UPON THE PROCESS OF TAPPING

The purpose of this experiment was to investigate to what extent an automatic or nearly automatic performance such as the described form of tapping with the right hand can be affected by the simultaneous expectation, and execution of an additional task involving attentional control and marked additional muscular effort.

This experiment differs from the previous one in two respects: First, there is no memory component involved in the sense of the subject working under a determination to remember the character of the additional stimulus for subsequent report. Second, the concomitant response involves an intensive motor component which requires a rapid postural adjustment and an actual expenditure of physical effort. Considering that there were only two alternative concomitant stimuli and that the respective responses differed only in their direction (movement backward and forward) we can say that we deal here with the rivalry of two motor tasks which differ in the degree of required physical effort and degree of possible automatization.

1. *Apparatus*

A handle of a large rheostat, such as is used to start machinery, was fixed by counteracting springs in a mid-point position. If the handle was pushed 45 degrees forward it would reach a *stop*, and the contact would activate an electric claxon. If it would be pulled backward the same number of degrees, the contact at the other *stop* would activate an electric bell. The mechanical resistance of the springs was about 5 pounds, which would be enough to stimulate the subject to return to the neutral position as soon as the claxon or bell has elicted a short sound. No leads had to be attached to the subject's hand. A metal tube was placed on the wooden handle of the rheostat so that it remained insulated from the rest of the equipment. The lead necessary for the contact with the subject's body was fastened with a clip to this tube and as soon as the subject touched the handle the circuit was completed. Two lanterns, a red and a green one, with a round luminous surface of about 3″ in diameter were fastened to a ring stand in front of the subject just above the lampboard used in the previous experiment.

In order to be able to differentiate between the tapping done during various phases of the same experiment two relays were added to the equipment. One was activated simultaneously with the stimulus and would convert the 6-volt wave to much larger 12-volt waves on the oscillograph. Such an arrangement permits the determination of the appearance and duration of the stimulus. The other relay was activated by the device for the concomitant response and would register, by a short interruption of the Alternating Current, the beginning and the end of the concomitant response. The equipment did not permit great precision for this relay. The beginning break occurs after the progression is nearly 3 degrees under way, and the second break occurs the same number of degrees before the return to neutral. However, such an arrangement permits us to establish relatively well the period when both hands were supposed to be in motion.

2. Method

(*a*) *The control experiment.* Before the details of the fundamental experiment were explained to the subject a number of practice trials as in the previous series were given. The only change in the conditions was the grasping of the rheostat handle with the left hand, instead of having this hand in a perfectly free position, generally upon the left knee. This was done in order to ascertain whether the change of posture as such, without any concomitant activity, will bring about some changes in the rate and other characteristics of the performance.

(*b*) *The experiment with multiple activity.* After the control sample was taken the subject was instructed:

> "When the red light appears pull the rheostat handle backward until the bell rings. When a green light appears push it forward until the claxon sounds. In both cases you must return to the original position as soon as you hear the bell or the claxon. The stimuli will follow one another in quick succession. Watch carefully and respond as fast as you can, tapping at top rate all the time. Refrain from remarks until the experiment is over."

Several practice trials for the left hand were given, the light stimuli being presented at a slower rate, so as to make definitely certain that the problem was well understood. Then several regular trials were given, but for practice purposes only. Following this, two experimental series were given.

The instructions were as follows: "Remember we want you to tap as fast as you can, and watch the light signals, so that the handle is properly operated. Ready, Go." After about one second of tapping the camera was started and immediately after this a stimulus was given. After the bell or claxon had sounded a signal light would indicate to the experimenter that the handle was returned to the neutral position, and the camera was stopped about a second afterwards. The subject would continue to tap, and after about one or two seconds, the next stimulus was given, in the same manner and so on until four stimulus-response situations were completed: two red and two green lights in various sequences. The practice series differed from the experimental series only in the order of presentation of the stimuli, and in that no oscillographic records were taken. The sequence of stimuli in the experimental series was: (1) Green, Red, Red, Green (A_1, etc.) and (2) Red, Green, Green Red (A_2, etc.).

To summarize: Ten subjects were used in this experiment. Each one was given two *normal* series, one at the beginning, one at the end of the sitting. To the same subjects two series with attention-motor concomitants were also given, each consisting of four SR situations in which a discriminatory motor task had to be performed with the left hand, while tapping at maximum speed with the right hand. The full records of nine of these subjects were analyzed, representing a total of about 4800 readings. The computation of the length of each double stroke presented another 1200 items, bringing the amount of items tabulated in this section to 6000.

Results

Inspection of the records and preliminary computation of data indicated a great similarity between the corresponding sections of each series. On this basis the data for A_1 and A_2, B_1 and B_2, etc. were pooled and presented as sections A, B, C, D (table IV). From an experimental standpoint each such section can be subdivided into three periods as follows:

(1) The period from the appearance of the concomitant stimulus up to the initiation of the concomitant response. We call this section of the experiment period X.

(2) The period coincident with the concomitant response—period Y.

(3) The period immediately after the concomitant response, covering the time required for five strokes, or about three-quarters of a second—period Z. This period is also to some extent a period of expectation of the next accessory stimulus.

The inspection of tables III and IV reveals the following facts:

(*1*) *The normal control.* The "normal" series in this group gave significantly smaller averages per stroke, and for most of the four variables, than the "normal" series in the first experiment. (Table IIIa and IIIb; compare with *Normal* in table Ia and Ib.) Whether such acceleration was due to tension in the left hand grasping the handle, or simply to change in posture and especially the position of the left hand was not further investigated.

(*2*) *Tapping with concomitant motor activity.* During the 12 to 15 seconds of tapping which was the duration of the whole series, the time per stroke increases steadily from 149.0 (A in period X) to 174.4 (D in period Z). The difference amounts to 25.4 milleseconds. In order to account for this decrement a "recheck for fatigue" was made on five other subjects who were instructed to tap continuously for 30 seconds. No additional tasks were superimposed or expected. The results for the stroke are given under the corresponding heading in table IV, and the full analysis is given in table IIIb. A further examination of the data shows that the average per stroke at the beginning of the 30-second period (147.0) is almost identical with that for the beginning of the concomitant response experiment, A in period X (149.0 ms.) and both are greater than the average for the *normal* stroke (139.5 ms.) under the same conditions. This difference, at least in part, must be attributed to the fact that the anticipation of a longer period of tapping may slow down the rate of performance at the onset. In the *normal control* (table IIIa and IIIb) the subject knew that the experiment would not last more than about 5 seconds, while in the others he knew that a task of longer duration

TABLE IIIA

Finger tapping (left hand grasping rheostat handle)

Number of subjects 10

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
Norm.	50.1	14.9	29.7	394	17.1	6.6	38.8	390	57.4	14.5	25.2	392	16.0	4.6	28.5	387	139.5	13.8	9.9	390

TABLE IIIB

Number of subjects 5 (Recheck for fatigue)

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
A (beginning)	53.6	9.5	17.7	65	15.3	3.9	25.7	65	63.8	6.2	9.7	65	15.2	3.3	21.5	65	147.0	11.8	8.1	65
B (15 seconds after)	57.1	12.1	21.2	61	18.6	4.5	24.0	61	67.7	9.2	13.5	60	18.6	2.6	13.9	61	159.7	15.4	9.7	61
C (30 seconds after)	58.9	8.9	15.1	57	22.3	3.8	17.1	58	63.6	8.3	13.1	56	21.2	3.4	16.1	57	163.0	9.9	6.1	57

Summary of tables IIIa and IIIb

Total number of subjects	15
Total number of strokes	573
Total number of photographic readings	2,294

TABLE IV

Tapping with concomitant motor response to an additional stimulus

Averages of time per stroke
Number of subjects 10

	Av.	SD.	V	N	BLKS.	ITEMS OMITTED
Normal	139.5	13.8	9.9	390	0	4

Motor concomitant
Number of subjects 9

SERIES	PERIOD X						PERIOD Y						PERIOD Z					
	Av.	SD.	V	N	Blks.	Items omitted	Av.	SD.	V	N	Blks.	Items omitted	Av.	SD.	V	N	Blks.	Items omitted
A	149.0	18.6	12.5	39	0	0	176.0	30.9	17.6	39	6	1	160.0	19.2	12.0	90	0	0
B	160.9	17.3	10.8	33	1	0	186.3	25.7	13.8	34	5	6	169.6	14.9	8.8	86	0	0
C	169.0	17.8	10.5	35	0	0	184.6	30.3	16.4	33	3	5	172.1	15.9	9.2	87	0	0
D	170.8	17.8	10.4	32	0	0	195.1	33.6	17.2	26	10	3	174.4	15.6	8.9	90	0	0

Recheck for fatigue
Number of subjects 5

	Av.	SD.	V	N	BLKS.	ITEMS OMITTED
A (Beginning)	147.0	11.8	8.1	65	0	0
B (15 seconds after)	159.7	15.4	9.7	61	0	0
C (30 seconds after)	163.0	9.9	6.1	57	0	0

Summary of table IV

Total number of subjects	15
Total number of strokes	1,197
Total number of items read	1,216

is given. However, the fundamental feature which concerns us at present is that at the end of a 15-second period of simple tapping we have obtained an increase of time per stroke from 147.0 to 159.7 milleseconds, or 12.7 milleseconds, while in the case of the motor concomitant experiment we obtain, for about the same period, an average increment of more than twice as much, i.e., 25.4; this exceeds by 9 milleseconds the increment for 30 seconds of continuous normal tapping. This appears specially significant when we consider that the 174.4 in D of period Z is an average of the strokes tapped *after* the four concomitant responses were completed, i.e., not involving any of the strokes *during* the concomitant responses. We can venture to state that the concomitant "attention-motor" task has influenced the general set of the organism during fifteen seconds in a manner exceeding the effects of 30 seconds of continuous simple tapping. The further analysis of the results will be undertaken by separate periods.

The period X (table IV) embraces the tapping strokes from the one during which the accessory stimulus appeared up to the stroke during which the response movement was about 2 or 3 degrees under way. It consists of about 3 strokes embracing a time from about 350 to 600 milleseconds. In the case of one subject, the period covered about 900 milleseconds, corresponding to 5 strokes, but this is an exception. This period includes a reaction time to light of the choice reaction type, possibly prolonged due to the anticipation of a complicated task (Boder)[4] plus the time for actual displacement of the handle of the rheostat for 2 to 3 degrees of the 45 degrees and return to be covered. It was decided to investigate whether the few strokes included in this period reflect in some manner the various phases into which this period could be subdivided.

There is first of all the stroke during which the stimulus strikes the retina, and the problem presents itself whether this illumination of the retina could possibly act in some way as a modifier of the rate of the tapping movement. We called this stroke the stimulus stroke and designated it by X_1. This stroke was computed by taking the element in which the stimulus appeared, viz., the top reversal and adding it to the following *down*, *bottom*, and *up*, values.

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[4] Paper presented at the 10th International Congress of Psychology, Copenhagen, 1932.

Then there is the last stroke in the period. This is really the stroke in which the concomitant reaction begins, at least in most cases, since the "break" on the film in the next stroke indicates the reaction well under way. We called this one the overlapping stroke, X_3. Then we have the stroke immediately preceding the overlapping stroke, which we termed the penultimate stroke, and designated by X_2.

Unfortunately our records do not present enough material for all the desirable comparisons. First of all, in spite of precautions taken the camera did not always gain enough momentum to make the stimulus stroke readable, although the film was satisfactory for all other purposes. Secondly, not all subjects made the same number of strokes during the same period, and the "stimulus stroke" would in some cases coincide in part with the penultimate stroke, or in a few cases even with the overlapping stroke.

We shall give here the comparisons of the stimulus stroke with the overlapping stroke as representing possibly a tendency which merits further investigation. The averages per stroke of each category are given with a statement of how many subjects show a tendency for an overlapping stroke longer or shorter than the stimulus stroke:

TABLE VA

	STIM. STR. X_1	OVERLAP. STR. X_3	N.	OVERLAPPING STROKES		
				Faster	Slower	Equal (±2 ms.)
A	154.2	141.8	6	5	1	0
B	177.2	176.3	6	2	2	2
C	169.6	170.7	7	2	3	2
D	172.7	172.0	7	3	3	1
				12	9	5

The tendency is for the overlapping stroke to be shorter than, or at least equal to, the stimulus stroke. If the contrary occurs, it is more frequently under conditions where fatigue or possibly the onset of an optimum rate could be inferred.

If we examine the tables and notice the significant reduction in rate for period Y and then refer back to the overlapping stroke, we can safely establish that this change in rate is not gradual but abrupt. It would indicate that the tendency to maintain a certain rate at a given moment is not impaired until the concomitant response is actually under way. This is corroborated by a more impressive number of comparisons available for the penultimate and overlapping strokes:

TABLE Vb

	PENULT. STROKE X_2	N.	OVERLAP. STROKE X_3	N.	X_3		
					Faster than X_2	Slower than X_2	Equal to X_2
A	152.3	13	143.1	13	10	2	1
B	164.2	12	158.9	12	6	4	2
C	163.1	11	164.3	11	6	5	0
D	171.4	12	158.6	12	5	3	4
					27	14	7

The foregoing data indicate rather clearly that there is a tendency for the overlapping stroke to be faster than the penultimate stroke, and again more so at the onset of the series than towards the end.

The overlapping stroke tends in 57 percent of the cases to be faster and only in 30 percent of the cases to be slower than the preceding stroke. The significance of this phenomenon is still more emphasized by the fact that the stroke immediately following the overlapping stroke, i.e., the first stroke in period Y (when the concomitant response is fully in progress) shows in the overwhelming number of cases a decrease in rate characteristic for this period. How account for this tendency of the stroke preceding and possibly overlapping with the concomitant action being shorter than its predecessor, and why should it appear more marked at the onset of the experiment than at the end?

It appears to the writer that the explanation of this phenomenon can be achieved by applying to it the principle of the Dominant experimentally formulated by Ukhtomsky (110) of Leningrad. (See Freeman (44), Physiological Psychology.)

Ukhtomsky found that the swallowing or defecation reflex of the cat can be accelerated by stimulation of unrelated regions of the cortex, v.g. the center for flexion of a limb. His collaborator, Ufland, found that the mating reflex of a frog (Rana temporaria) can be accelerated by means of a mechanical, chemical, or electrical stimulation of any part of the body. In Ukhtomsky's own words:

> "A sufficient intensive and stable excitation, which takes place in a center at a given moment, acquires the significance of a predominant factor in the work of other centers: it attracts to itself the stimulation from remote centers, and impairs the capacity of another center to react to its usually adequate stimuli." (p. 61).[5]

At first sight it appears that we deal here with a modification of the principle of drainage and we shall discuss it more extensively in our final chapter. At present we shall only investigate how such a principle of interdependence could be applied to our findings in this investigation. If we return to the fact that the overlapping stroke tends to be faster than the preceding one, we can infer that at a certain phase between the stimulus and the response, at a moment just before the adequate response was given, this stimulus was capable of accelerating the process of tapping already in progress. The fact that this phenomenon of dominance appears more outspokenly at the beginning of the series would suggest that a new index of fatigue could possibly be sought in the decrease of the summation capacity of a fatigued center. However, there is another alternative. Further experiments show that a decrease in rate could be considered a practice effect. The subject reaches his optimum rate at which continuous work can best be maintained only after a certain period of much faster performance. He starts at a nearly maximum rate, which however, can not be maintained for more than a few seconds, while the slower rate is maintainable for a longer period and may even lead to some recuperation, so that temporarily the rate will tend to become again somewhat faster. If, under such conditions, the phenomenon of the Dominant (or in other words, the phenomenon of summation) tends to disappear, one could venture to infer that with practice, or after a warming-up period the active centers show a rise in threshold against overstimulation from indirect sources.

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[5] Translated from the Russian by D. B.

The Y period. Tapping during concomitant action with the other hand. The Y period occupies from about 300 to 900 ms., and involves from one to three strokes (tables IV and VIIy). The tapping during this period reveals some peculiar characteristics, only rarely observed in our other experiments. We have classified these irregularities under two headings: *syncopations* and *blocks*. In the former, the sequence of the components, bottom, up, top, down, is maintained, but the numerical value of one of them appears from two to six times larger than the average of these same components in the rest of the strokes in the same record. If such an increase in value of the variable occurs at the top or bottom reversals we can safely infer that the subject has simply stopped. However, if it occurs during the progression time we have no way of telling whether the subject stopped, or moved very slowly, or has continued the reciprocal movements at a narrower width so that the bottom and top contacts did not occur. If the latter should happen, which is highly probable in some of the cases, it would indicate the complete loss of proprioceptive control for that moment. Such increases in length of the supposed progression period are rare, and in no case exceed 100 ms.

The significance of this occurrence lies in the fact that certain phases of intentional action can be disrupted and reëstablished sometimes in time intervals of less than one-tenth of a second. Such occurrences of abnormal increase in time for one of the components may be compensated at least in part by a decrease of time for the others, so that the time per stroke will not be excessively large. However, it may not be compensated for, and then the sum of the four components of the stroke does become excessively large. In the computation of the averages and SD for the distributions in which such outstandingly long strokes are present, these were at first tentatively omitted. If the obtained results indicated that they fell within the range of the *average plus 3 times the SD* they were put back into the distribution and the average and SD were recomputed, including these strokes.

If, however, these values would be larger than the average plus 3 times the SD, they were definitely omitted and classed as such (see table IV and Appendix). Such omissions (with one exception)[6] never exceeded four items for the whole array of data, other than this period, from which the averages, SD's or V's were computed. They are rare in normal experiments and are few in fatigue experiments. Their relative abundance in the Y period, i.e., during concomitant action acquires the significance of a special feature of such a situation.

All that was said for the "syncopation" holds to a great extent for the other irregularity, the "blocks" which also only rarely occur in all other investigated situations, but are relatively abundant in the Y period, i.e., during concomitant action. By a block we mean an occurrence which registers, for instance, a bottom reversal, progression, and again a bottom reversal. Here it is definite that the subject has lost his bearings, at least where guidance by contact, pressure and definite sense of position is concerned. He reverses the progression without reaching the opposite limit of the tapping frame. It is not a stop during which the concomitant movement is performed, but a cramping or disorganization of the tapping pattern. None of the nine subjects is free of one or the other described types of irregularity during the Y period. But there are great individual differences as to the frequency of their occurrence (table VI).

Another characteristic of the Y period is the great variability of tapping. Only in six of the 72 experiments was the left hand response accomplished during one single complete stroke, which is in such cases a definite syncopation. In four of the 72 cases the concomitant response caused a single continuous block in the

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[6] This exception refers to the computation of the av., SD., and V for the Normal, Downward progression, the experiment described in this chapter, when data from two groups of experiments given to the same subjects under similar conditions in the same sitting were pooled, yielding a total of 394 cases. The number of items omitted from this frequency table was seven, or less than two percent.

TABLE VI

Analysis of individual records of each of the nine subjects for period Y (during concomitant motor response) as to irregularities described in the text

NUMBER OF RECORD	Av. = 177.5 σ = 29.6 P.E. = 20.2		Av. = 186.7 σ = 25.8 P.E. = 17.4		Av. = 182.0 σ = 32.2 P.E. = 21.7		Av. = 194.6 σ = 34.1 P.E. = 23.0		TOTAL IRREGULARITIES PER SUBJECT			
	A_1	A_2	B_1	B_2	C_1	C_2	D_1	D_2	Block	Single stroke	Diff.>σ	Total irregularities
311–312	*24*	*91*	*129*	Str (329)	Blk	*87*	*44*	Str (347)	1	2	4	7
315–316	*28* Blk	9 Blk	15	17	5	*45*	2 Blks	Str (197)	4	1	1	6
323–324	*33*	11	Str (236)	*28*	*48*	*29*	9 Blk	4	1	1	3	5
327–328	*23* Blk	Blk	Str Blk (287)	*142* 2 Blks	11	Str Blk (412)	Str 2 Blks (127)	Str 2 Blks (141)	10	0	1	11
339–340	Blk	*65*	Str Blk (187)	*317*	*65*	*207*	Str (443)	Str Blk (199)	3	1	4	8
343–344	*72*	*112*	*52*	*58*	*232*	*12*	Str Blk (249)	*117*	1	0	7	8
347–348	15	14 Blk	7	*22*	4 Blk	4	*28*	Str Blk (158)	3	0	0	3
351–352	*66*	*33*	13	14	11	*73*	*48*	*168*	0	0	5	5
355–356	*22*	19	*144*	*33* Blk	*40*	Str (150)	*48*	16	1	1	4	6
									24	6	29	59

Blk = block. Str = only stroke in whole period, with duration in ms. given in parentheses.

Other numbers indicate maximum difference between the 2 to 4 strokes involved in the whole Period Y. Those in italics indicate *Diff.* > PE.

tapping for the whole Y period. In 56 of the 72 individual records for the Y period we find one or the other of the described irregularities, but in only ten records does the irregularity last for the whole period of concomitant action. In 46 cases the irregularity is combined with one or two regular strokes, although possibly of a slower rate. The variability in the Y group can best be characterized by the following facts: in 54 of the 72 records two or more complete strokes were available; the maximum difference found between such strokes exceeds in 35 cases, or 67 percent, the PE of the distribution, while 29 of these same differences (54 percent) exceed the SD for the distribution. That this percentage is extraordinarily large can be substantiated by the fact that in 15 records obtained in the control experiment such differences between neighboring strokes were found only in 6 percent of the non-fatigued subjects, 15 percent after 15 seconds work and 32 percent after 30 seconds work. A further characteristic of the irregularity of the response in the Y period is that in a number of cases a regular stroke or syncopation appears preceded or followed by a block. This would mean that the pattern of the tapping performance has broken down during the concomitant response, but that the disturbance has not lasted as long as this response. There was an interference with the fundamental task, but only for part of the time.

We can conclude that the tapping with the right hand is variously affected by the concomitant response executed with the left hand, that the effect is not uniform during the whole period, and that the character of the irregularity varies from subject to subject and from experiment to experiment in the same subject. The fact that we have sixteen records (or 22 percent) in which irregularities are not observed would indicate that we do not deal here with a combination of activities which are per se physically incompatible.

The period Z or the period immediately after the completion of the concomitant task, corresponding also to a period of expectation of a new stimulus. For the sake of uniformity the values for this period were computed from the five strokes following immediately after the stroke in which the break in the time line would indicate the return of the left hand within three degrees of the neutral position. (In seven of the seventy-two records, only four strokes were available.)

The inspection of table IV, section Z indicates: (1) That the time per stroke during the period immediately following the concomitant task is shorter than during the performance of the concomitant act, (2) that it tends to be longer than the stroke in the corresponding X period, i.e., the period preceding the concomitant task, and more so at the beginning stage of the series than at the end, (3) that this rate of the Z period does not differ more than three milliseconds from the subsequent X period in which the new stimulus appears. The latter findings would indicate that the rate of tapping during expectation of a stimulus is not materially different from that in the period of stimulation itself. This inference must be made with some reservations since between the Z period and the following X period, there is an interval of about one to two seconds and there are no introspective reports available as to whether the Z period was directly a period of active expectation or more a period of recuperation from the preceding, Y period. The following facts, however, remain established: the decrement in rate following the disturbance in tapping during Y neither subsides nor increases during about three seconds, and within the limits of our experiment the rate suffers further progressive decrement with each new concomitant performance, the degree of decrement being greatest at the beginning of the series.

To summarize: (*a*) A superimposed task of "attentional-motor" character causes a decrement in the rate of the described performance which after about five seconds reaches the fatigue level of fifteen seconds of continuous normal tapping, and after ten seconds exceeds the decrement obtained after thirty seconds normal tapping. This fact can not be taken as a matter of course, since it is obvious that the postural conditions in holding and manipulating the handle involve a great deal of tension, which usually is considered a facilitating factor for a number of performances. It is possible that Freeman's findings of the decremental character of hypertension may be corroborated by this experiment although we will have to attribute to it to some extent a retroactive character, since the decrement in period Z occurs *after* the operation against the springs, not during it.

TABLE VIIx

Attention-motor concomitant

Period X. Number of subjects 9

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
A	54.4	19.9	36.6	39	18.9	6.8	36.0	39	56.8	17.3	30.4	39	18.4	4.4	24.1	39	149.0	18.6	12.5	39
B	61.9	9.9	16.0	32	20.0	4.7	23.7	33	63.2	19.2	30.4	33	19.3	5.1	26.3	33	160.9	17.3	10.8	33
C	62.8	12.5	19.9	33	23.9	8.0	33.4	34	65.8	15.0	22.8	33	22.2	6.2	28.1	35	169.0	17.8	10.5	35
D	64.6	7.7	11.9	32	22.5	4.7	21.1	32	62.4	16.8	26.9	32	20.9	3.8	18.2	31	170.8	17.8	10.4	32

Summary of table VII, period X

Total number of subjects	9
Total number of strokes	139
Total number of photographic readings	249

TABLE VIIy

Attention-motor concomitant

Period Y. Number of subjects 9

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
A	67.2	18.3	27.2	39	25.2	11.9	47.3	39	63.7	23.9	37.6	40	20.9	7.1	34.1	38	176.0	30.9	17.6	39
B	72.2	20.4	28.2	38	23.7	6.2	26.1	36	67.0	25.2	37.7	39	22.0	7.1	32.4	38	186.3	25.7	13.8	34
C	69.9	23.0	32.9	37	25.6	6.2	24.4	37	70.4	29.7	42.2	37	23.4	6.6	28.2	35	184.6	30.3	16.4	33
D	72.3	19.1	26.4	27	29.0	9.4	32.6	28	72.6	22.6	31.1	29	22.8	5.9	25.9	28	195.1	33.6	17.2	26

Summary of table VII, period Y

Total number of subjects	9
Total number of strokes	132
Total number of photographic readings	565

TABLE VIIz

Attention-motor concomitant

Period Z. Number of subjects 9

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
A	62.8	16.0	25.5	90	20.2	6.3	31.2	90	59.1	17.0	28.8	90	19.0	4.5	23.7	89	160.0	19.2	12.0	90
B	65.9	12.1	18.4	85	21.3	4.6	21.4	86	64.7	16.8	26.0	86	19.7	4.1	20.9	86	169.6	14.9	8.8	86
C	66.4	11.0	16.6	86	22.4	4.8	21.4	87	63.8	18.8	29.5	87	20.4	3.9	19.3	86	172.1	15.9	9.2	87
D	65.8	13.9	21.1	89	24.4	4.9	20.2	89	63.9	16.6	26.0	90	21.8	3.6	16.6	90	174.4	15.6	8.9	90

Summary of table VII, period Z

Total number of subjects	9
Total number of strokes	353
Total number of photographic readings	1,406

Retroactive facilitation of tapping due to muscular tension has been postulated by Dresslar (30) in 1894, although this tension was induced in a rather roundabout way. Dresslar's contention was that intensive mental work is accompanied by muscular tension which persists for some time after the mental task is over. If during such a period a tapping test was given to the subject, the rate was found to be faster.

(*b*) Another attempt to explain the disorganization of the tapping pattern during the concomitant task, and the lack of complete recuperation during the subsequent period, could be made in evoking the drainage theory of McDougall and his followers. This could be done only by a further elaboration of the drainage hypothesis, adding a number of assumptions which could be supported at present only by further inferences, or analogies. The following questions would have to be formulated and answered: (1) What accounts for the fact that the disorganization of the tapping pattern during the concomitant response is not always of the same character; i.e., it reveals itself sometimes predominantly in the form of blocks, sometimes in the form of syncopations, and sometimes in great variability? The answer to this question could be that the character of the concomitant response is not always of the same intensity, nor does it always coincide (due to conditions of the experiment or individual reaction times) with the same component of the stroke. We could argue that an onset of a concurrent reaction during a reversal may lead, possibly, to an extension of this reversal time without loss of form of the pattern, while the onset of the reaction during the progression may have as a consequence the loss of orientation in the movement, i.e., a block. Unfortunately, our technique did not offer any possibility of establishing these facts. (2) How can we explain that in some cases (to be sure, only a few) the tapping pattern is apparently not disorganized, at least in the described forms? The answer could be that there is nothing in the drainage theory which prevents the assumption of the following two alternatives; (*a*), that energy may be drained from a different source than the one in action; in our case it would be not from the sensory-motor system involved in the tapping, but from the action systems of the heart, lungs, intestines, or postural muscles not involved in the procedure, and (*b*), that the active pathway has not taken all the available energy and that there was enough available for the concomitant task. Such a situation would present a new aspect of the problem of learning, especially of bimanual skills and the like. We could then venture to infer that the difficulty a subject experiences in performing a bimanual task, although the parts for each hand are mastered separately, lies in the inability of the organism to drain off from all other regions at one given time the energy necessary for both tasks together, or in an inability to distribute this energy in the proper proportions. Learning under such conditions would involve also the components of proper energy distribution. The various sport activities and the playing of musical instruments seem to present corroborating evidence for such an inference.

To what extent the theory of drainage or the hypothesis of the Dominant should be evoked in explanation of the described phenomena will be discussed in the final chapter.

Returning now to table IV: Period Y shows a marked decrease in rate when the period of the first concomitant response is compared with that of the second, and especially with that of the fourth (Y_A vs. Y_D). This decrease is only momentary, lasting during the concomitant response and subsiding immediately, with only a slight after-effect. On the other hand, table VB on page 41 shows that the overlapping stroke (X_3) tends to be shorter than the preceding stroke, but that tendency is much less in the D series than in the A series. If we attempt to explain the decrement by drainage, and the increase of speed in the overlapping stroke by the principle of the Dominant, then it would appear that with fatigue the threshold for the dominant phenomenon is heightened, while for drainage it is lowered. In other words, a fatigued sensory motor system is less capable of absorbing accessory stimuli and is more ready to be deprived of its energies by other systems which happen to be thrown into activity.

From the foregoing it appears well established that during simultaneous activity either of the two performances may be intermittently affected in rapid succession both by inhibition and facilitation.

A further analysis of table IV (section Y) indicates that during the concomitant response the variability of the stroke in tapping is in three of the four series greater than in any other situation so far studied.

The films show that one and the same subject during a period of less than a second may manifest a great variety in his responses. As table V indicates, a normal stroke may be preceded or followed by another stroke which would exceed the same by more than one SD or may appear together with a block or a syncopation. Even in an excessively long stroke only one or two components may show great irregularity while the rest would appear to be of normal duration. Such results lead to the conclusion that a concomitant task may interfere with another motor task only at certain moments of the performance, while at other moments that interference may be absent or negligible. We may also conclude that some components of the concomitant task have a greater power of interference than others and that a voluntary performance, such as tapping, is not always equally affected by one and the same concomitant task.

Note: The coefficients of variability corresponding to tables III to VII are plotted on charts on pages 70 and 72.

VI. TAPPING WITH A TOOL AND CHANGES IN PERFORMANCE DUE TO CONTINUOUS WORK

1. The problem

It appeared evident that this new method of measurement of motility might throw some light upon the modifications of motility due to continuous work. Attempts to obtain samples of continuous finger tapping for a minute or more under our conditions revealed the following obstacles. The subjects in their zeal to coöperate would hit the sides of the tapping frame with such force that after a minute of tapping the index finger would appear badly bruised. Independently of the fact that some subjects were exceedingly sensitive the condition in general appeared to be beyond control and except for a few exploratory experiments the idea of obtaining "fatigue" records on bare finger tapping was abandoned.

Preliminary experiments with the tapping board revealed great differences in the time values for the various components of the movement when instead of tapping with the bare finger a metal stylus was used. It was also observed that in tapping or hammering with a tool in the hand or even when repeatedly knocking with the bare fist, wrist movements are soon abandoned in favor of elbow movements. It is only when the elbow is strapped, or when a great deal of attentional control is given to the movement, that relatively pure wrist movements can be elicited. However from the onset of the experiment we were interested in the accomplishment of a task by means of free hand movements permitting the subject to use all the muscle groups at his disposal for a given task, in a freely chosen posture. It is very possible, the difference in the numerical relationship between the elements of the stroke without corresponding change in the rate is due to transfer of action from one gross muscle group to another, as from wrist tapping to elbow tapping. The neurological and psychological implications of this problem are of great theoretical and practical significance; but as far as casual introspective reports indicate, the subjects were never aware of such changes, at least they could not recall any intentional shifts in their modes of action.

For these reasons it was decided to investigate with more detail the difference between finger tapping and tool tapping and also to ascertain what possible influence continuous work might have upon the performance.

2. Apparatus and method

It was our intention to obtain a tool in which the position of the various muscles would be as similar as possible to the anatomical constellation of the finger tapping as performed in our previous experiments. After a number of preliminary trials with various devices a tool (called Bar 5) was adopted. It was made of one piece of galvanized iron, about 14″ long, $\frac{5}{8}$″ wide, and $\frac{3}{32}$″ thick. The total weight (after drilling one hole in it) was about 130.5 grams. At the tapping end about $2\frac{1}{2}$ inches of the length were doubled over. This had a twofold purpose: (1) It counterbalanced to a great extent the longer part of the bar. (2) It prevented the finger from slipping. At three inches from the tapping end a $\frac{5}{16}''$ hole was drilled and counter-sunk about $\frac{1}{2}$ inch in diameter so that it made a comfortable bed for the distal phalanx of the index finger.

The distance from the hole to the proximal end was about $7\frac{3}{4}''$, which was long enough to pass just behind the wrist joint. This was important because any shorter tool would rest with its proximal end upon the flesh, causing possible discomfort and bruises, and undue pressure in the direction of the wrist joint. It must be recalled that all tools intended for hammering are designed so that the free side of the handle protrudes beyond the closed fist. (This is not so with tools intended for rotation such as screw drivers.) The instructions were the same as for bare finger tapping except that the subject was given the tool and instructed to hold it so that the distal phalanx of the stretched index finger would rest in the hole. Several preliminary trials were given for about five seconds each and then the "normal" samples were taken.

For the fatigue experiment the subjects were instructed to tap at the fastest possible rate and were told that the experiment would last two minutes. Photographic samples from the oscillograph were taken at the beginning and at fifteen second intervals so that nine records per subject were obtained in the fatigue experiment in addition to the "normal." From five subjects also "recuperation records" were obtained in which the samples were taken after a rest period of one minute following the two minutes of continuous work.

3. *Results*

The data obtained on ten subjects seem to indicate that the rate of tapping is not necessarily affected by the use of a tool such as the bar described (see table VIII; norm. and A). On the other hand the values for the four components of the stroke are entirely different. The reversal times, especially for the bottom reversal, are shorter, the progression times are longer and this in spite of the fact that with increase of distance from the carpal

TABLE VIII

Bar 5. Tool tapping "fatigue experiment"

Number of subjects 10

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
Norm.	41.6	12.2	29.3	110	34.4	6.3	18.2	110	52.3	14.6	27.9	111	33.5	6.0	17.9	110	160.9	18.9	11.7	111
A*	42.5	12.7	29.8	108	33.8	5.2	15.5	108	51.7	15.7	30.4	109	34.2	5.3	15.6	109	160.0	20.8	13.0	108
B	45.4	15.6	34.4	108	34.7	7.1	20.4	107	59.5	14.0	23.4	108	33.8	6.1	17.9	107	172.3	20.0	11.6	108
C	48.9	17.1	35.0	109	36.1	5.8	16.1	107	59.9	15.2	25.4	109	36.7	6.3	17.1	109	180.4	23.2	12.9	109
D	45.8	17.2	37.5	101	37.1	8.4	22.6	99	60.1	15.5	25.9	101	38.3	8.3	21.7	101	180.6	22.5	12.5	101
E	49.9	15.3	30.6	94	38.7	6.9	17.9	93	61.6	15.1	24.5	94	39.1	7.5	19.2	92	186.9	19.3	10.3	92
F	52.2	19.5	37.3	93	40.5	9.5	23.4	93	62.8	14.8	23.6	93	39.4	7.8	19.9	91	193.9	21.3	11.0	92
G	50.7	18.0	35.5	92	40.3	9.3	23.0	90	62.1	17.4	28.1	92	41.7	11.0	26.5	91	194.3	22.7	11.7	92
H	53.8	16.0	29.7	90	40.8	7.1	17.5	90	63.3	16.8	26.5	89	39.7	7.6	19.1	89	197.4	25.3	12.8	90
I	52.4	15.8	30.1	90	42.0	7.0	16.6	88	62.8	12.7	20.2	88	39.8	6.5	16.3	87	194.8	19.7	10.1	87

* A at beginning of two-minute period. Subsequent samples taken at 15 second intervals.

Summary of table VIII

Total number of subjects	10
Total number of strokes	990
Total number of photographic readings	3,960

joint the absolute value of the distance between the two *stops* of the tapping device corresponds to a smaller number of degrees of rotation. It would appear reasonable that the progression time upwards with a tool would be longer than with the bare finger. The records indicate that this is true. But the progression downwards is also much longer with the tool than the progressions downwards with the bare finger. Our averages for downward progression with the bare finger fluctuated in round numbers from 15 to 31 ms., while in tool tapping the range of averages for the same downward progression is from 32 to 42 ms. Apparently the grip of the tool adds other factors to the situation than weight, and if the gravity factor is operative at all, it is so overshadowed by the others that it becomes of secondary significance. This latter statement is supported by another set of rather unexpected results. If we compare the data in table VIII for the top reversal with the same for the bottom reversal we observe that the average for the top reversal is about 10 ms. (about 25 percent) longer than the bottom reversal, and that this absolute difference maintains itself all the way through. Now the top reversal is the position at the upper limit of the frame, the position *against* gravity, and nevertheless, in this form of tapping at least, the reversal time from an up to a down movement, i.e., in the direction of gravity, is consistently longer than the reversal time for the movement against gravity.

Three problems may then be formulated: (1) What accounts for the shorter reversal time in tool tapping than in finger tapping? (2) What accounts for the longer progression time with a tool than the progression time in tapping with the bare finger? (3) What would account for the longer top reversal?

It is apparent that a different technique than ours would be necessary to approach these problems experimentally. The slow motion picture camera presents at least one method to be tested. At present we present some considerations with reference to the first two problems. First the factor of tension. The tool is held firmly in the hand, the index finger and thumb extended; the other three fingers are flexed and press the tool towards the palm. This tension must be maintained since otherwise the strokes against the sides of the tapping frame will throw the tool out of position. We find that the rate of normal tool tapping for the group of ten subjects is identical with the rate of normal finger tapping under similar postural conditions.

We reproduce here only the averages for the components, taken from tables Ia and VIII.

Finger tapping—Normal (10 subjects)

BOTTOM	UP	TOP	DOWN	STROKE	N SUBJECTS	N STROKES
62.8	17.3	64.8	16.6	161.0	10	237

Tool tapping—Normal (10 subjects)

BOTTOM	UP	TOP	DOWN	STROKE	SUBJECTS	STROKES
41.6	34.4	52.3	35.5	160.9	10	111

We see then that the tension factor if operative has not changed the rate of tapping as given by the average for the stroke. But the ratio between the variables has greatly changed. We have an increase of speed in reversal with a decrease in the speed of progression. If we consider tension as a factor directly operative upon both functions, i.e., reversal and progression, we will have to state that while tension is capable of accelerating the interaction between neural centers, to bring about the rapid shift of action from agonist to antagonist, it damps the speed of muscular performance as such. If the distance of progression is insignificant as in finger oscillation the increase of reversal rate may greatly overshadow the lag in progression and give a final increase in rate for the performance. If, however, the progression-distance is larger, the speed of reversal may only compensate for the produced lag in movement and the rate will remain unchanged. We are then again confronted with the problem of drainage, or some other variety of interdependence in a specific form. We observe that tension in one set of muscles (provided tension is operative) may have a detrimental effect upon the efficiency of simultaneous contraction of other muscles without affecting the processes of higher neural levels (reversals) which are involved in the performance. Such an inference would suggest that this interdependence or drainage is more effective upon the components of the concomitant activity which belong to the same neural level, and manifests itself to a lesser extent in the case where the neural levels are not the same; i.e., a cortical process is more liable to interfere with another cortical process than with a motor act, the same as the latter would interfere more with another motor act than with a cortical process. However, it must be made clear that the validity of the problem can be tested only upon motor performances that do not involve postural incompatibility, since otherwise it would be a fallacy to attribute the inhibition of one act to drainage or any form of higher interdependence.

We shall now attempt to account for the acceleration of the reversal, which we attribute to a more rapid shift of stimulation from center to center. The already discussed principle of the Dominant formulated by Ukhtomsky suggests one alternative explanation. This investigator, working with animals under mild anesthesia, has established that an afferent stimulus may precipitate a discharge of activity in another supposedly unrelated pathway, provided that this latter is ready for action. In our case, the tension produced due to the necessity of gripping the tool may cause an additional influx of proprioceptive stimulation which would flow in the direction of the centers in readiness, i.e., those of the antagonist to the one in action, and produce in this manner a shortening of the reversal time.

It however does not seem indispensable to assume that tension must exercise a direct and counterposed influence upon both functions, i.e., the reversal and the progression. It may be possible to prove in the light of physiological evidence relating to muscular movement that other conditions being equal a prolonged progression (for whatever extraneous reason) may be made to account for a shorter reversal time, or a shorter reversal (for whatever reason) may be made to account for the longer progression. If a factor could be found that accounts for the increase in speed of reversal, then an attempt could be made to explain the decrease in rate of progression as a consequence of this increase. Or, vice versa, if the decrease in speed of progression could be accounted for by some extraneous factor, then the increase in speed of reversal could be taken as a consequence of this decrease. What has to be borne in mind is that in either of these assumptions only one phenomenon must be taken as the consequence of the other, and that no reciprocal influence should be postulated.

With this in mind we shall investigate whether the principle of the refractory phase can not throw light upon the problem. Our stroke consists of a pair of antagonistic movements. It is really a double stroke. If a movement is made upward there is no reason why the reversal time for the next downward movement should be longer than the time of relaxation of the muscles which have just completed the movement (let us call them the agonists). But suppose the antagonists have been in action before. Then the relaxation of the agonists by itself will not bring about the initiation of the movement in the opposite direction, unless the refractory phase of the antagonists is over or so far advanced that a movement can be initiated. The recuperation of the antagonists can take place during the time consumed by the progression of the agonists plus the time required for the subsequent relaxation of the agonists. If the duration of these two periods together is too short the refractory phase will outlast them by some amount of time which will be added to the reversal time. If however the time taken for the movement and relaxation of the agonists is sufficient for the recuperation of the antagonist, then the decrease of speed during the progression due to some exterior factor, will curtail correspondingly the length of the arrest of movement for the reversal. Our data on page 57 show that this might be so.

On the other hand the problem may be approached from a somewhat different angle. Let us consider the sensory cues. This has to be done with caution in view of some recent physiological experiments. It has been proved by Briscoe, 1932, (20) on cats, that upon electrical stimulation of the peripheral nerves leading to antagonistic muscle groups a response will be elicited which depends on the postural condition of the limb. For instance, if the limb has been put by the experimenter in a position of passive flexion, simultaneous stimulation of the flexor and extensor nerves will give extension. If to the contrary the limb were put in a position of extension, the opposite will happen under the same conditions of stimulation. However, none of the investigators would venture to deny that in the intact organism afferent stimuli from cutaneous and deep receptors play an important if not a predominant part in reciprocal limb action.

Comparing the sensory cues under both conditions of our experiment we find that (all other factors remaining the same) we have in finger tapping a sensory cue of pressure at and around the small surface of about ½ sq. cm., at the place of contact. There is also some stimulation in the metacarpal joint, in which the finger may still rotate after the progression has been inhibited by external mechanical means. In tool tapping however, with the bar clenched lightly between the palm and the finger, the shock of the stroke reverberates upon a wider area which in total gives a stimulus of much higher intensity. This may reduce the reaction time, but with the consequence that the innervation may fail to spread over the sufficiently large number of neuro-muscular units necessary for contraction at a faster rate. The difficulty lies in the fact that the averages for reversal times are all less than 75 ms., and for some experiments do not exceed 30 ms. All these values are very small in comparison with the usual findings in reaction time experiments. It is in general a question whether in rhythmical action each response serves as a stimulus for the next response, or whether rhythmical action consists in coördination of such a kind that the components are released in proper sequence and properly timed as the result of a determination, or *Aufgabe*, without any specific stimulus for each of them. (See Fearing (43).) This however would not exclude the possibility of an act being modified by increase of intensity of one of the sensory concomitants, and if such a factor has increased the speed of reversal in our tool tapping, the lag in the progression is possibly accounted for.

Recheck for recuperation

When the films for the first six subjects in the fatigue experiment were developed and analyzed it was decided to take an additional sample to investigate recuperation after two minutes of continuous work. There were still 4 subjects to be tested, and one subject was added later to bring the number for the recheck experiments to five. The procedure with these subjects was identical with that for the rest, except that an additional sample was taken one minute after the continuous work period. When, at the end of the two minutes tapping, the order *stop* was given, subject was told: "Now take a rest for one minute, then we want a few seconds more of tapping." Table IX gives the data for this group. We apparently deal here with some subjects working at a faster rate and the absolute values are less for this group than for the whole 10 subjects. However, the trend of the working curve is the same, giving the largest decrease in rate during the first minute, and nearly a plateau for the second half of the experiment.

Analyzing the data for the experiment "one minute after" (J), as given in table IX and also in charts 1 and 3, we find the following facts: (1) the time per stroke is back to normal, (2) the progression time shows no recuperation and even tends to be somewhat longer than at the end of the two-minute period. The whole gain in rate is obtained by an unusual decrease in the reversal time which falls to a level not observed in any previous record. If the reversal is a function of interaction of nervous centers (or half-centers), or is at least predominantly neural, while the progression is fundamentally *neuro-muscular* in character, we can then infer that one minute of rest was, under the conditions of our experiment, more than sufficient for recuperation of the higher centers but of no noticeable benefit to the effector mechanism *per se*. We shall return to the discussion of this problem in the next chapter.

VII. VARIABILITY OF PERFORMANCE

Variability of performance has of late become a prominent subject of investigation in fatigue studies. The most recent work is

TABLE IX

Bar 5. Fatigue recheck available

Number of subjects 5

	BOTTOM				UP				TOP				DOWN				STROKE			
	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N	Av.	SD.	V	N
Norm.	38.4	9.6	25.1	57	33.2	6.6	19.9	56	44.7	14.2	31.8	57	32.8	6.5	19.9	57	147.0	16.0	10.9	56
A*	36.2	11.2	30.8	57	33.5	5.3	15.8	57	40.7	14.8	36.5	57	33.9	4.0	11.9	57	143.5	16.9	11.8	57
B	41.9	14.6	34.7	55	33.5	8.3	24.9	55	54.2	11.8	21.8	55	32.1	6.8	21.0	54	159.1	14.6	9.2	54
C	44.3	18.9	42.6	54	37.3	9.2	24.6	54	53.9	11.0	20.5	54	37.4	8.0	21.3	54	171.2	15.1	8.8	54
D	35.5	17.9	50.5	51	38.8	12.0	30.8	51	56.6	11.3	19.9	50	39.3	10.4	26.6	51	167.8	15.3	9.1	51
E	40.3	18.3	45.4	49	39.8	8.4	21.2	49	60.1	9.7	16.1	49	41.9	10.4	24.7	49	180.0	15.8	8.8	49
F	45.0	20.3	45.2	49	41.0	12.5	30.4	49	58.3	11.5	19.8	49	40.8	9.4	23.2	48	184.4	13.6	7.4	48
G	46.6	19.6	42.1	49	41.7	12.9	31.0	49	56.9	13.4	23.5	49	40.3	15.1	37.4	48	183.1	13.6	7.4	48
H	48.7	16.2	33.2	46	39.9	9.4	23.6	46	63.9	9.2	14.3	44	38.4	9.6	34.9	45	189.1	17.2	9.1	46
I	49.2	16.1	32.7	48	39.8	8.9	22.5	47	60.5	11.9	19.7	47	37.9	8.0	21.1	46	185.8	11.9	6.4	45
After 1 minute rest																				
J	28.5	12.4	43.6	59	42.2	8.0	18.8	59	36.5	14.4	39.5	59	38.7	5.9	15.2	57	146.0	17.9	12.3	59

* A at beginning of two-minute period. Subsequent samples taken at 15-second intervals.

Summary of table IX

Total number of subjects	5
Total number of strokes	567
Total number of photographic readings	2,282

Manzer's, 1934, (70). Analyzing ergograph tracings, he finds that "the relative variability of work done with fatigued muscles is three times the variability of the work done with the unfatigued muscles." One thing has to be kept in mind about ergographic studies, at least those in which variability so far has been analyzed. Ergographic work is unidirectional, i.e., the finger working against a load in flexion is assisted by the load during extension. Sure enough, the extension with the help of gravity is by no means a passive process. In protecting the finger from a painful pull the return must be much slower than it would be in the case of the weight acting upon the finger which has been previously relaxed. These protective activities of the organism

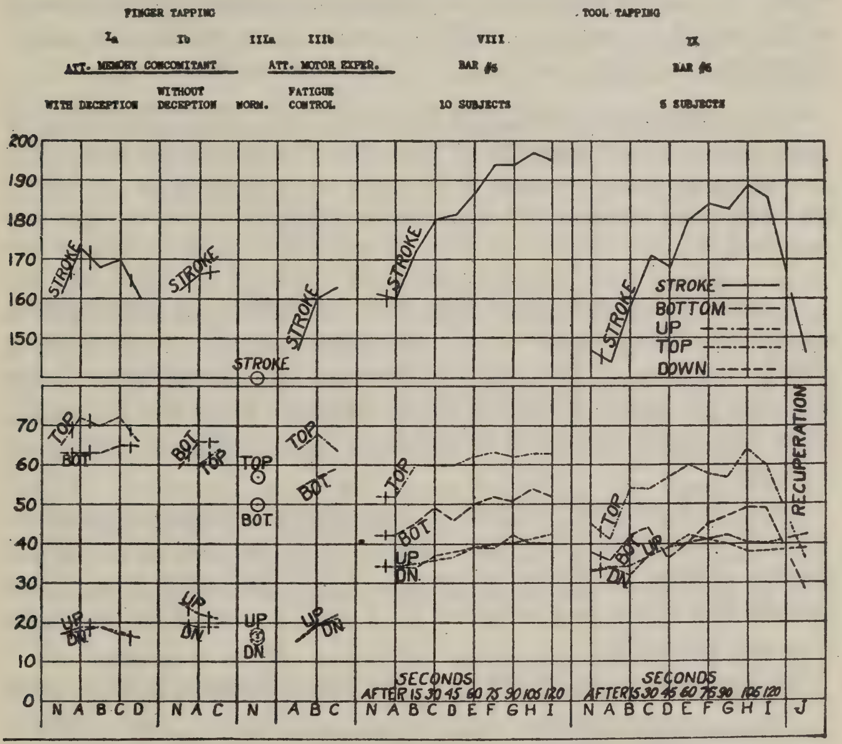

Chart 1. Graphs of Averages in Milliseconds of the Stroke and Its Four Components Obtained Under Various Experimental Conditions

are of quite a different order than the action against the weight, and as long as they are not analyzed we must consider that the ergographic data present only one phase of the performed operation. The variability studied is then in terms of one component only. Weinland, 1927, (113) and Manzer, 1934, (70) find that relative variability increases with fatigue. Thorndike's findings, which do not refer directly to fatigue, but simply to continuous work, would indicate a possible decrease in variability as a consequence of continuous work. To quote from Manzer:

> "In his experiment of drawing 3810 lines with eyes closed, trying to make each line two inches long, Thorndike found that the variability around the subject's own central tendency was reduced from .11 inch to .08 inch, when the first 193 responses are compared with the last 202 responses."

It must be remembered that this variability in Thorndike's experiment refers again to only one aspect; length. Variation in speed, duration of the interval between the completion of one line and the start of the following, pressure upon the paper, all these aspects if measured might have given different indices of variability.

In chart 3 we have plotted the variability of the stroke and of the four components for the normal and the fatigue series including the sample J which corresponds to the work after rest of one minute and which was preceded by a work period of two minutes (see also table IX).

In agreement with Holzinger (59) and Manzer we have used as a measure of variability the *coefficient of variation* ($V = \frac{100\sigma}{\text{Av}}$) since our data fall in the category which Holzinger (59) describes as "series where the means differ considerably in size and where the variation relative to the mean is therefore important" (p. 117).

As the curves indicate, there is at the beginning a similarity in variability between the two reversals, the same as between the two progressions. After fifteen seconds the variability of the top reversal falls appreciably and maintains itself at lower levels than any of the four components, while the variability for the bottom reversal proceeds in the opposite direction, reaching its height after 45 seconds of work, a period during which the most rapid decrement of the stroke rate in terms of duration of the stroke occurs. After 60 seconds, when the rate seems to have become stabilized for the next minute (see tables VIII and IX), the variability of the bottom first remains unchanged and then at the end of 105 seconds decreases almost to its initial lower level.

The variability coefficients for both progressions, *up* and *down*, hold more closely to each other and with the exception of sample B and H follow a course between the high variability indices for the bottom reversal and the low variability indices for the top reversal.

At the end of the two-minute period we have the following constellation; the variability indices for *bottom*, *up*, and *down*, have all returned to nearly their original level, at which they were at the beginning, while the variability index for the top reversal is markedly below its original par level. The next record (J) is taken after one minute rest. Comparing the variability for the recuperation sample (J) with that for the end of the two-minute period (I) we observe that the variability of the progressions has slightly decreased, more nearly approaching the level for the beginning, while the indices for top and bottom have gone up, the latter to a level never reached before, and the former to a level about 43 percent above its value at the beginning of the experiment.

We turn next to the variability indices of the rate itself, i.e., to the variability of the stroke. The fundamental characteristic of this curve is that it maintains itself consistently lower than any of the four variables, leading to the conclusion that the fundamental condition for uniformity of performance is the plasticity of function of the agents involved in the production of its various components so that the lag in speed of one muscle group may be compensated by accelerated activity of others, or that a lag in speed of the agonist produces more efficient recuperation and therefore more rapid action of the antagonist. This would permit maintenance of a nearly constant rate, or, if changes due to practice or fatigue occur, they will take place in a gradual manner. This lower variability for the stroke than for any of its other components holds for 43 group records out of 45. Even in the two exceptions the coefficient of variability for the stroke only reaches the level of one of the components and there is no case in which the coefficient of variability for the stroke would be greater than for any of the components.

The examination of individual records undoubtedly corroborates Weinland's findings as to great individual differences in variability of performance. Such individual differences may greatly influence the data for a group provided the number of subjects is small. It suffices to compare graph IX (chart 3) with graph VIII (chart 2). The former presents data for 5 subjects, 4 of which belong among the 10 subjects represented in graph VIII. The trend of the curves is roughly the same, their initial position is almost identical, but the curves for the smaller number of subjects show greater irregularities during the period of continuous work. There is a wider divergence between the measures for the top and bottom reversals and a more erratic trend of the measures for the progressions. However, the stroke curve preserves its position at a level definitely lower than any of the components.

Let us return now to table IX and graph IX (chart 3). At the end of twenty seconds the differences in variability for the various components compared with their intial variability could be tabulated as follows:

(*a*) *Percentage of change of variability at the end of 2 minutes compared with variability at the beginning*

BOTTOM	UP	TOP	DOWN	STROKE
+6%	+42%	−46%	+77%	−46%

(*b*) *Recuperation compared with beginning in terms of variability*

BOTTOM	UP	TOP	DOWN	STROKE
+42%	+19%	+8%	+28%	+4%

(*c*) *Recuperation compared with end of 2-minute continuous work in terms of variability*

BOTTOM	UP	TOP	DOWN	STROKE
+33%	−16%	+101%	−28%	+92%

It is evident that the change in variability of the stroke does not necessarily reflect the amount and direction of change in variability of all the components. If the variability at the end of the two-minute period is compared with the initial variability in terms of the stroke, we have a *decrease* of the coefficient of 46 percent; if, however, the rate of the downward progression is considered, which may well be done in an ergograph experiment, the coefficient would indicate an *increase* of variability of 77 percent.

If we compare the variability in the recuperation experiment with the initial variability, we again face a multiplicity of findings. Taking the whole stroke as a criterion, we are within 4 percent of recovery, while the values for the bottom reversal and both progressions would lead to quite different conclusions.

If the recovery period J is compared with the end of the two-minute work period, i.e., period I, we again have increase of variability, judged by the stroke and reversals, while the progressions both show marked decrease in variability. We see then that one and the same performance may give concurrently various indices of change in variability depending upon the particular characteristic or component of the performance measured.

Any explanation for such an occurrence must be tentative until further exploration has been made. It seems, at least for some aspects of work, the fatigue effect can be better detected after a short rest than directly at the end of the work period. There seems to be a kernel of truth in the remark that fatigue after a long walk is more noticeable when one sits down for a moment, and that after a brief period of sleep one may feel more tired than before lying down. It would seem that the fatigued organism is better capable of continuing an activity in progress, than of rebuilding the activity after some interruption.

Something similar was found by Weinland (113):

> "Among the results stated in regard to difficulty of adjustment under different conditions, there is one of particular importance: it is that the first-quarter variability following *work and a rest* is higher than the first quarter variability when the subject is fresh; and that this initial variability of the second work curve changes with the duration of rest. These facts suggest the possibility of measuring the amount of previous work done, or degree of recovery from work by a test of variability." (p. 18)

But this generalization is not without exception. Weinland finds that for work with the arm ergograph (24 lb.) the variability after a rest of 10 seconds is greater than after a 5-second rest period, and that this "brings a difficulty into the interpretation of the more general facts stated above." In venturing an explanation, Weinland states:

> "We must assume that after a 5-minute rest, first-quarter variability is lower than after a 10, either because a balance between the loss of warming up and recovery from work is being passed through, or because of a chance difference." (p. 16)

Our data would suggest that if more than one component of the complex act has been measured, the expected greater variability would have been found; just as a component with lower variability could possibly be detected in the performance after 10 minutes rest. This seems important both from a theoretical and practical standpoint. Weinland seems to expect a possible relationship of variability and accident proneness; he also sees some industrial disadvantages—in greater degree of variability in a given individual. He states: "Individual differences are in some cases striking and emphasize the conclusions in Chapter I that workers should be tested for variability as well as for output" (p. 64).

His explanation of variability, in the face of his experimental findings with direct electrical stimulation of the muscle is as follows: "The increase of variability with fatigue is due rather to a loss of control than directly to the exhaustion of the muscle."

If we consider that in a number of instances in our experiments the change in variability of the reversals, is greater than the corresponding change for the progressions, we may have found a measure to determine when the exhaustion has affected the higher centers and when it is to be found in the muscles, viz., as a result of accumulation of fatigue products.

As to the validity of a test for variability, as a personal trait, to be used in applied psychology we can only state that such validity has to be established by the usual statistical tools, and a "variability" correlating negatively with one criterion of efficiency may possibly give a positive correlation with another pertinent trait. The significance of a test for variability in psycho-pathology and clinical neurology may also appear reasonable. But as this experiment has shown, the possibility of obtaining contradictory generalizations is very great when only one or even two characteristics of a complex performance are analyzed. It seems very plausible that if further measurements could be added to the present experiment such as a measurement of tension, etc., their degree of variability would not necessarily coincide in amount and direction with the values already obtained. At each given moment we apparently deal not with a single variability of the organism, but with variabilities, sometimes apparently independent, sometimes with a tendency to positive, or negative correlation. The theoretical possibility of such a state of affairs was accepted by Woodworth when he discusses the relation between the "total variability" of the compound in relation to the "component variables" (121).

VIII. THEORETICAL DISCUSSION

We shall now attempt to state briefly the theoretical implications of the present investigation.

They could be dealt with under the following headings:

1. The psychological problem of voluntary muscular action.
2. The psychological implications of concomitant activity.
3. The physiology of rhythmical movement.
4. The problem of inhibition and facilitation.

We shall not attempt to give an historical review of these

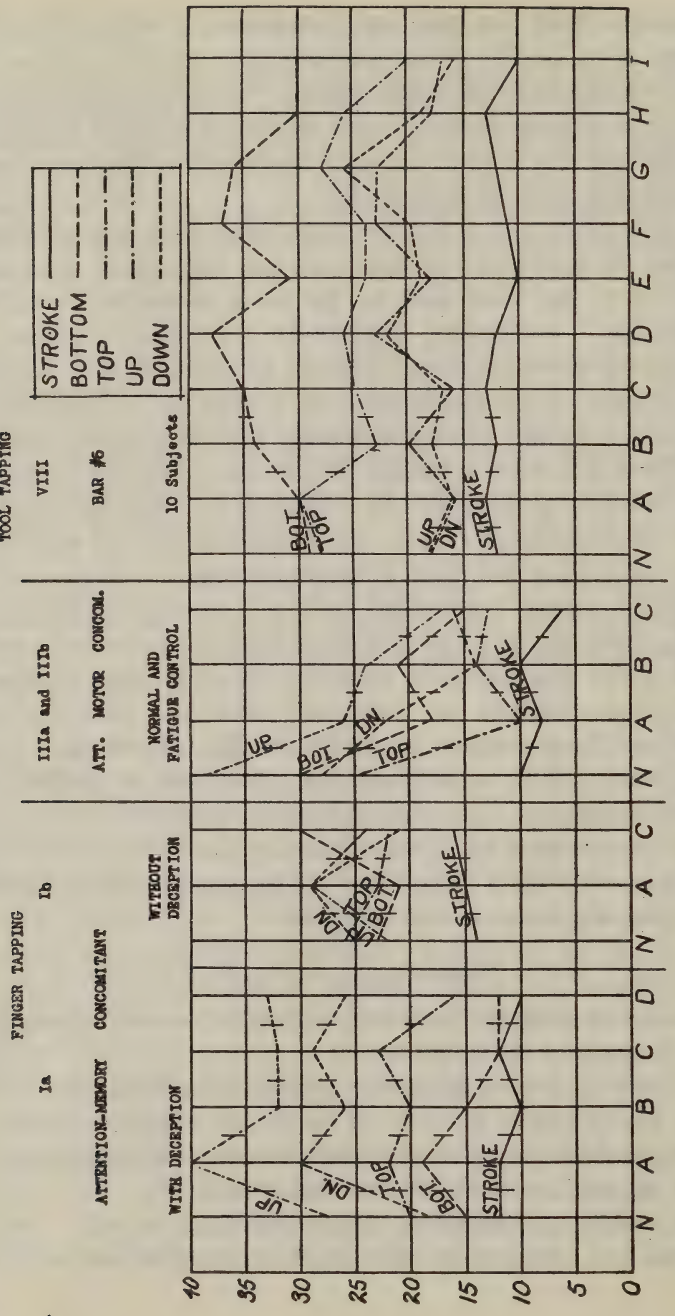

Chart 2. Graphs of the Coefficients of Variability $\frac{(100\sigma)}{\text{av.}}$ Obtained under Various Experimental Conditions

problems, but rather to clarify the present day status of these inquiries, and their bearing upon the presented research.

1. *The psychology of voluntary movement*

The problem does not involve the inquiry as to freedom of will in general. It refers only to the character of stimulation and the experiences involved in the operation of a movement, which we call voluntary; endeavoring in this paragraph to keep as far as possible from physiological speculations, leaving these for the second part of this chapter.

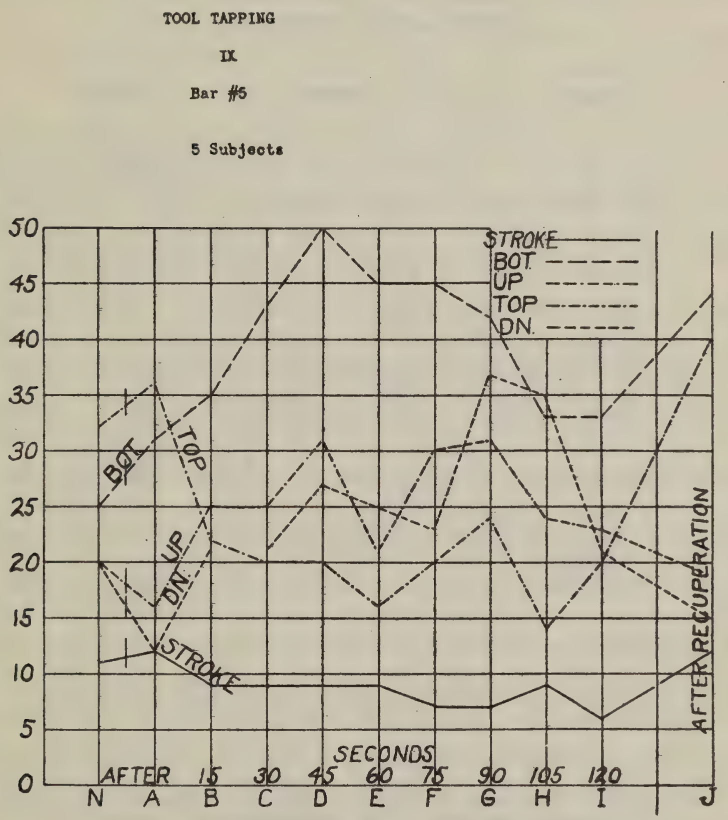

CHART 3. GRAPH OF THE COEFFICIENT OF VARIABILITY $\frac{(100\sigma)}{\text{av.}}$ OF THE STROKE AND ITS FOUR COMPONENTS IN THE TOOL TAPPING EXPERIMENT ON FATIGUE WITH A RECHECK FOR RECUPERATION

Taking for granted that visceral phenomena can be modified by means of conditioning in Pavlow's sense, we still are able to work out a hierarchy of movements which can be based on the greater or less participation in the process of that modality of experience which we call intention. We might subdivide all known motor phenomena occurring in the human body into two groups, those which possess a proprioceptive concomitant which can become conscious, and those without the same. Hunger pangs may serve as determining cues for the desire to eat, just as position of the retinal image serves as a cue for location of objects in space,

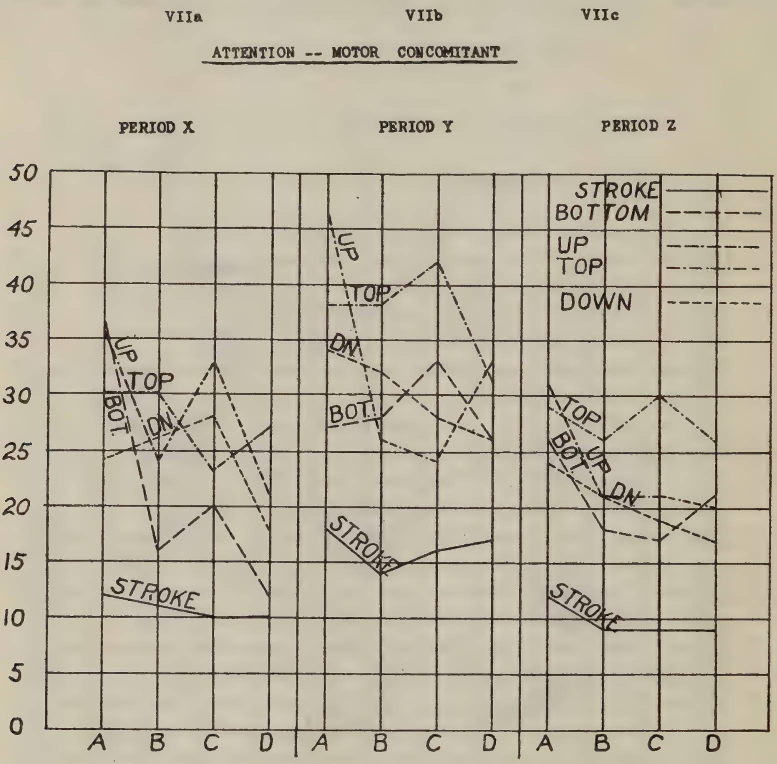

CHART 4. GRAPH OF THE COEFFICIENTS OF VARIABILITY $\frac{(100\sigma)}{\text{av.}}$ FOR THE STROKE AND ITS FOUR COMPONENTS OF EACH OF THE THREE PERIODS (X, Y, Z) IN THE EXPERIMENT WITH THE ATTENTION-MOTOR CONCOMITANT

but we are as unable to obtain direct knowledge of the hunger pangs as we are unaware of the retinal quadrants stimulated at any moment by the rays reflected from an object in the field of vision. Only motility which can be made an object of immediate experience is capable of being voluntarily initiated, maintained, and inhibited. To be sure, the organs of voluntary action can also serve as reflex apparatus, but the reflexes which involve such muscular groups as the somatic muscular apparatus are capable of intentional inhibition, at least after some training; sneezing may be inhibited as a matter of good manners, and the acrobat, standing with one foot on a rope may use his arms, the other foot, vertebral column and head to substitute for the response to a cue from the semicircular canals and retina which would usually elicit the reflex of setting down the other foot on *terra firma.* Voluntary motor activity may be of static or kinetic character. The person may hold his arm in a stretched position or may move it rhythmically back and forth. Both processes can be initiated intentionally, but here immediately emerges the question of limitation of the will by physiological factors. A reaction can not occur instantaneously, and the hundred yard dash will never lose its temporal quality. Wundt and his students have shown that the sensory and motor type of reactions which were considered as determined by constitutional characteristics of the subjects can be induced by instructions, but in the uninformed individual the character of the volitional act will be to some extent determined by conditions which will influence his mental set so that his thoughts would be directed predominantly toward either the expected stimulus or the reacting finger. We are dealing here with a phenomenon of fundamentally psychological nature, and the contributions of systematic introspection can not be ignored in this situation. What arouses doubt is the generalizations often made in a doctrinal fashion on the basis of introspective reports of a few subjects. They reveal all too often the bias of a school and the fallacies of a particular investigator.

The most extensive work in the field of experimental investigation of voluntary processes was begun and is still continued by Ach and a host of his pupils and followers. The researches of Ach were directed first to establish the existence of a *determining tendency* of "that peculiar after-effect of an act of will, which evokes the realization of an experience in the sense of intention."

The strength of this tendency is measured by the derogatory effect of a concomitant volitional task upon performances of various degrees of habituation. And vice versa: the strength of automatization of a learned performance is measured by the amount and complexity of a concomitant act that can be performed without upsetting the automatism (Mohnkopf, 1933).

An automatism or automatized action is defined by Ach as an activity which occurs on the basis of "associations formed through experience" but at present occurring with hardly any participation of consciousness. Due to the regularity of the process, its fundamental characteristics fade from consciousness, which may be again attracted to the process when some irregularities set in.

On the other hand we have the approach of the Gestalt school. The amount of research on action performed by this group is exceedingly small in comparison with their studies in the sensory field. Lewin (66) considers that Gestalt psychology has introduced a dynamic element into the concept of will. The experiments of Ovsiankina, 1928 (90) which show that interrupted activities tend to be renewed with increased intensity as soon as the disturbance is removed, lead Lewin to conclude: "Der Durchführung der Vornahmehandlung liegt also ein *bedürfnisartiges Spannungssystem* zugrunde, das von sich aus ohne äusseren Anreiz zur endgültigen Erledigung drängt" (p. 13).[7] He speaks of a special *structure* of action units and he proposes a quantitative evaluation of such structures on the basis of the intensity of this drive-like urge for completion of the interrupted task. Levin bases on this fact his theory of compulsions. If the results of Ach's experiment be taken behavioristically, there appears little difference between the "determining tendency" and the "drive-like urge." Both theories seem to be corroborated by our findings in chapter two, where after a block or other characteristic disorganization of the tapping procedure, the tapping regains its regularity of pattern often in less than half a second, although at a rate otherwise obtainable only under conditions of moderate fatigue.

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[7] "The execution of an intended act is based upon a drive-like system of tension, which urges the completion of the act without any further exterior stimulus." (p. 13)

There is another important psychological feature of voluntary movement which must be mentioned here. It appears obvious from introspection that a voluntary movement requires much less conscious control than a static muscular activity such as holding the arm stretched out at shoulder height. It seems that a voluntary change of posture which interferes with the force of gravity is an active interference not with gravity as such, but with proprioceptive reflexes or automatisms which tend to maintain the skeletal muscles in a tonic state below excessive tension. It seems that it is the sensory process which appears to be interfered with when a hypnotized subject maintains his stretched arm at shoulder height for a prolonged time. As it will be recalled our fatigue experiments had to be undertaken with a tool precisely for the reason that pain from continuous striking with the finger on the metal bar would become intolerable after about one minute. The fact that some authors found transfer of fatigue when after a period of work with one hand, the performance would be transferred to the other, does not indicate that this particular fatigue was necessarily central, but possibly that in relieving one hand from action we have not diverted to the same extent the load from the muscles of the back, the neck, and other muscle groups commonly involved in the maintenance of the working posture of either hand.

The question now arises, how such automatisms are formed, how do these "associative abstractions" occur in a psychological sense?

First of all introspections about automatic actions such as tapping are really retrospects. It seems hardly possible that a subject introspecting during tapping would really consider that he was unaware of his hand being in motion. Now as to the separate phases of reciprocal action, it would depend on the frequency at which proprioceptive stimuli are still perceived as separate. If the normal initial tapping rate is about eight per second it would amount to about 16 kinetic elements per second, which corresponds nearly to the number of exposures of the motion picture projector necessary to produce a stationary picture. Is it not possible that we deal here on the proprioceptive side with a similar phenomenon of fusion?

There is another situation similar to this which appears to us more than a plain analogy. According to the Hering color theory complementary colors arouse in the retina processes of opposite character. If a pair of complementary colors are presented in quick succession on a color rotator the action of the color receptors is suspended and a gray is perceived. This gray we would compare with the proprioceptive experience of movement which is always present during fast reciprocal action; what is suspended as a perceptual phenomenon is the discrimination of the separate half-strokes and their direction.

2. *The psychological implications of concomitant activity*

Little remains to be added about this topic to that which has already been said in the previous paragraph.

If the perceptual experience is so simplified by fusion, then it is not difficult to explain why other acts can be attended to concurrently: the reason being that a simple experience such as the perception of movement in a joint is incapable of commanding the total attentional attitude, thus permitting the care of additional items. The fact that not all concomitant tasks can be performed without upsetting the automatism may be explained by the fact (postural interference excluded) that the total of both experiences exceeds the amount that can be attended to, and the whole process may break down.

It is the same as when the summation of two subliminal stimuli may give a perceptible stimulus or when, as in Ebbinghaus' memory experiments, the addition of three syllables to a list of seven increased the number of repetitions required from one to thirteen (see Garrett (52)). Another illustration may be found in the digits test: if one reads to a normal adult subject for immediate repetition six digits at a rate of one per second (Stanford-Binet test, year 10), an errorless repetition is obtained. If, however, nine digits are given, the subject usually fails, his errors beginning much before the sixth digit, which he could have reproduced right if only six digits were given.

The fact that in retrospection there is little remembered about the details of an automatic act, simply indicates that the act contained little attentional cues, and that therefore there is little to recall. The figure-ground principle of Gestalt may be evoked here as indicating that the figure element was very meager. The fact that an act did not absorb our attention is by no means proof that the occurrence was *"subconscious"* as Bliss as early as 1892 would have us believe.

We must make it clear, however, that especially in the simultaneous performance of two motor tasks the incompatibility may be due to postural difficulties rather than to mental factors such as described above. If such be the case with some of our subjects in the two-hand experiment, it is noteworthy that the recuperation from the postural shift was unusually fast. Whether it is Ach's determination factor or Lewin's drive-like tension system, it is evident that the recuperation is as quick as the return of a spring to its normal size as soon as the pressure is removed.

The conclusion would be: postural condition being taken care of, two acts can be simultaneously performed if the total perceptual mass does not exceed the span of attention. Otherwise both actions are distorted unless one is definitely inhibited.

The fact that there were great individual differences in concomitant performance, and differences in the record of the same subject, can be attributed in the first case to individual differences in capacity, and to fluctuation of attention in the second case.

3. The physiology of voluntary rhythmical movement

The idea of rhythm involves two concepts, which are not always consistent in processes called rhythmical. One concept is that of time, predominantly a muscial concept. Eight bars of a waltz may be perfectly rhythmical, without involving two successive identical muscular performances. What makes music rhythmical is the timing of accentuation, the preservation of the time pattern independently of the pattern of activity involved in the production of one melody. As stated before, above the level of very primitive music the successive muscular operations of the player are anything but repetitive. On the other hand more minute examination of the duration of each of the repetitive movements such as in walking and tapping indicate that the fluctuations in time around a certain average may be rather significant, without being perceptually detected. Psychologically we are greatly interested in the physiological mechanism of voluntary successive movements but not of the type which, like walking, are associated with the fundamental maintenance of life, but of the kind where the spontaneous voluntary component is obvious. For instance, Klinger (62) in 1931, working in Wirth's laboratory has established that the shortest interval between two successive movements with the right and the left hand to be given as a response to one stimulus corresponds to about 30 ms. What does that mean from a psychological standpoint? There are two alternatives: (1) That the second movement, say that of the left hand, is a reaction to a proprioceptive and tactual stimulus from the right hand, or (2) that both hand movements are released from the cortex as a single response but are differently timed. In the first case, we deal with a reaction time situation. But to what stimulus? A reaction time as low as 30 ms., as far as we know, has never been obtained under conditions of the usual reaction time experiment. The experiments which appear nearest to the desired situation are those of Dodge (27) and Misbach (80) especially the latter. The former author does not give his reaction times for the finger reaction, while for the lid reaction there appears to have been introduced an additional stimulus (sound). The description of the experiments is ambiguous and in addition the records indicate reaction times significantly longer than the cited 30 ms. obtained by Klinger as an interval between the two hand movements.

In Misbach's experiment we deal with choice reactions and so the data are not comparable, either, with those of Klinger. Although Misbach has found that

> "knee-jerks resulting from tendon blows occurring not more than .3 sec. before nor more than .1 sec. after the voluntary stimulus effected consistent reduction of voluntary reaction time," (p. 280)

and rightly assumes that "the reaction is peculiarly subject to facilitation by proprioceptive impulses," we have, nevertheless, no data available as to reaction times obtained directly in response to proprioceptive stimuli and we are unable, to date, to decide whether reaction times as short as 50 ms. are really obtainable in overt locomotor behavior.

We have then the second alternative, that of the successive response being a result of an intentional timing process. If such timing refers to two successive right and left hand movements, a performance which in no way enters as indispensable in our every day activities, and is not concerned with the reflexes of equilibrium (which is generally the case in foot movement), the question as to the mechanism by which this timing is accomplished remains open. It will probably have to be classed together with the inquiries on psychology of skills, gestures, speech and the like where a motor sequence pattern is acquired by means of learning.

The mechanism of timing in continuous reciprocal movements appears more intelligible. We may say that in walking, the minimum time between two flexions of the *same* leg would be determined by the fastest possible action of the extensor muscles in the *same* leg. This is well observed in rope jumping of children, or to some extent in the gallop of horses. This is also the fundamental characteristic of the tapping movement. The time per stroke as von Kries has already pointed out, will be determined by the rate of contraction of the agonist, the time of reversal and the rate of contraction of the antagonist. If more than one stroke occurs, an additional reversal time must be included between the return to the starting position and the initiation of the new stroke. The explanation of this kind of rhythmical movement seems less difficult in the light of available physiological material. We shall only state the possible alternatives.

(1) After the initiation of the movement the change of direction is determined by proprioceptive and possibly exteroceptive impulses. In our experimental situation we could say that contact and pressure against the upper bar of the tapping frame, possibly sight, and the sound, of the knock, could serve as cues in addition to the proprioceptive stimulus. If this be true we are

again presented with the problem of the possibility of reaction times as low as 28 ms., which is the lowest mean average we have obtained for reversal times in our experiment.

(2) However, if we take the second alternative and assume the existence of some kind of a timing mechanism in the central nervous system, then we have at our disposal a number of facts from earlier as well as from recent literature in support of such views. First we have a statement of Sherrington,[8] 1900, which we quote from McDougall (71, p. 177).

> "The reflex spinal movement, if prolonged, tends always in my experience, to be of *alternating* character. . . . A movement from the initial posture of rest to another posture is obtained; this is the primary movement, and with it the reflex reaction, if of a short duration may come to an end. But with prolongation of the reflex action the posture first attained is not maintained. A second movement, opposed in sense to the former, sets in. And this alternation of posture may be repeated a number of times. . . . In their phasic alternation of condition the motor cells pass through periods of discharge and quiescence; the contraction and the relaxation contemporaneously affect antagonistic muscles," so that . . . "inhibition as well as excitation seems to play a part." (p. 177)

What part is played by afferent stimuli in the timing of alteration of reciprocal movement? If we consider walking this, according to Fulton (50), may also occur "when all the afferents of the limb have been severed." The same holds for the scratch reflex.

In a recent report on reciprocal innervation in spinal and decerebrated cats, Ranson and Hinsey (96) make the following statements:

> "The state of neural balance in the spinal centers is an important factor in determining the type of reflex response obtained. Sometimes in the course of a given experiment this balance will shift so that a stimulus that at first gave a crossed extensor response will later give a response in which flexion predominates, although the strength of the

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[8] Sherrington, C. S.: The Spinal Cord, *Schäfer's Text-Book of Physiology*, 1900, Vol. 2, p. 831.

stimulus and the position of the electrode have not been changed. . . ." (p. 12)

"Reflex reversals may be obtained by changes in the passive posture of the limb. In general it may be said that if a limb is passively extended its spinal centers become set in favor of flexor reflexes and if it is in a flexed posture the neural balance of the spinal centers favors extensor responses."

Under the heading "Complex Reactions" the authors continue:

"*Demarcation Between Flexor and Extensor Responses.* This demarcation is not sharp. A pure flexion and a pure extension are completely dissimilar and diametrically opposed reactions. But they are not isolated; they stand at the extreme ends of a more or less graded series of reactions in which evidences of the exitation and inhibition may be seen in both of the antagonistic muscles. The contraction phase may occur in the two muscles simultaneously or alternately during the stimulus. Or the contraction of the extensor muscle may occur as a rebound after the cessation of a flexion-producing stimulus. In spinal cats that have been allowed to live for several weeks after the transection, these diphasic reactions can be obtained with ease. This is of importance because it shows that the mechanism responsible for their production is complete within the spinal cord. In making this statement we do not lose sight of the fact that in the intact animal this spinal mechanism is profoundly influenced by and constantly under the control of higher centers." (p. 13)

That the condition in the limb may be a factor in determining whether flexion or extension shall occur, and that this is now necessarily dependent upon afferent stimulation, has been shown by Briscoe, 1932 (20) when she stimulated simultaneously the nerves supplying the antagonistic mucles of the deafferented limb (cat). If the foot was previously put in a position of flexion, stimulation of both nerves would give extension, the limb returning to a position of "neutrality." If the limb was put in position of extension, and both nerves were stimulated, flexion would return the limb to "neutral." As the author puts it: "apparent lowering of threshold in response to stretch takes place even in the nerve-muscle preparation."

As is now definitely established, and commonly accepted, reciprocal movement does not require complete relaxation of the antagonists while the agonists contract. What does happen is a phenomenon of co-contraction (recent investigations by Dodge and Bott (28), Ranson and Hinsey (95), Wilson (119), and others), the direction being determined as Sherrington has pointed out by the "algebraic sum of their opposite effects" (see Fulton, p. 132).

The further explanation of reciprocal movement involves the theory of "half-centers" formulated by Graham Brown (1911).

There is a flexor "half-center" and an extensor "half-center" in which the neurones for the corresponding muscle groups originate. One and the same proprioceptive stimulus would be capable of inhibiting one center and reinforcing the other so that the antagonistic activities could operate with proper synchronization.

If we now remember that at least in some forms of rhythmical activity the afferent innervation is not the exclusive determinant of the direction of movement at a given moment, we may venture to assume that these half-centers could also receive a common cortical stimulation which will maintain their activity according to a time pattern predominantly determined by the nature of these half-centers and by the conditions of the musculature as such. Considering the view of Ranson and Hinsey that "the flexor and extensor half-centers appear to exert an inhibitory influence on each other," this common cortical stimulation of both centers could be reduced to direct or indirect cortical control of only one of the half-centers.

Let us return now to our problem of reciprocal hand movement. A learned act is different from a reflex, or deflex as Ziehen (122) has lately defined the term, in the sense that it is a behavior pattern established by experience, and not preëstablished by philogenetic development. There is no reason for assuming that the mechanism of retrieving the finger from a false key on the piano, is in its fundamentals much different than retrieving the finger from a hot iron or from a shock producing electric appliance. The process of automatization would consist in establishing such reflex-like behavior patterns, in which after a "cortical release," the process would go on much like a spinal cord affair or at least like an act taken care of by centers at lower brain levels. But it would be perfectly erroneous to assume that the *whole process* after once being initiated could remain at the mercy of the lower centers, or would exclude the significance of proprioceptive cues.

This is emphasized by Fulton, and by Ranson and Hinsey (see p. 80). Let us then make an attempt to investigate how conscious control could operate in such an automatized process as tapping. In our discussion of conscious participation in the act we have attempted to show that the rate of about 16 proprioceptive impulses per second could easily produce a phenomenon of fusion into unity or for that matter a curtailment in clearness of perception of the separate components of the movement due to their subliminal duration. What remains is a blunt experience of movement which apparently does not fade entirely from consciousness at any time during the task. But it would be a great fallacy to abstract in any way the tapping performance from the posture indispensable for the maintenance of the process. The arm in our experiment was not supported and the posture had to be coördinated so that the finger should remain inside the boundaries of the frame. The resistance to fatigue and the overcoming of the slight cumulative pain due to the forceful striking against the frame, which appeared unavoidable at so fast a rate, all had to be taken care of by the determination to act, in the sense of Ach, and could hardly escape consciousness for longer periods than determined by the usual pattern of fluctuation of attention. What gives the act a character of an automatism with "abstracted association" (Ach) is apparently only that part of the performance which occurs at a rate that makes the separate component unfit for perception, or which consists of components so much alike among themselves, that each one separately does not stand out in any way so as to leave a distinguishable memory trace.

To sum up: a voluntary rhythmical movement is a learned behavior form. As a result of practice a rate can be developed which gives to some elements of the performance the character of subcortically controlled functions. This maintenance of the movement is accomplished by a large constellation of factors such as the carry over of a determination taken before the beginning of the task, exteroceptive cues, and proprioceptive stimulation from regions of the body directly and indirectly involved in the process, such as pressure in the joints, and muscular strain.

4. The problem of inhibition and facilitation

The concepts of inhibition and facilitation are based on the general observed fact that the response of muscles both in the intact organism and in the muscle-nerve preparation is not uniform in intensity or duration in spite of the identity of the applied stimuli. So Verworn (111) has shown that contraction of a muscle can still be evoked by means of stimulation of its motor nerve after the response to the corresponding central excitation has disappeared, as a result of prolonged stimulation. The seat of the inhibitory process is in such a case postulated to be in the central nervous system. The source of this intracentral inhibition is not definitely established. Older theories assumed the existence of special afferent neurones, the only function of which was considered to exercise an inhibitory effect upon certain centers. This is at present considered antiquated. According to Brücke (22) there is hardly a sensory nerve which is not able to arouse not only a reflex or a tonic condition but also a state of inhibition. This statement is somewhat ambiguous, since a sensory *nerve* could contain specialized inhibitory and specialized excitatory fibers belonging to different neurones. But when we consider that Brown and Fulton postulate in their scheme (see p. 82) one single neurone which would simultaneously excite to action one half-center while inhibiting the other, we may infer that Brücke's term nerve really refers to the neurone.

More controversial is the problem of special inhibitory centers. The clinical picture of decerebrate rigidity, paralysis agitans, Parkinson's disease, etc., are taken by some authorities as proof that with the cortical injury the inhibition of lower centers has been removed, i.e., that the cortex is among others the seat of inhibitory centers. Beritoff to the contrary, argues that this hyperexcitation is a result of continuous irritation due to the lesions (see Brücke (22)). Whatever the divergences of opinion as to details may be, the principle of central inhibition appears as an accepted physiological fact.

Aside from inhibition of central origin the possibility of peripheral inhibition is also accepted. But while central inhibition is postulated not only as a manifestation of fatigue, but also as an important factor in reciprocal movements and in general as a part of all forms of motor adjustment, peripheral inhibition is dealt with exclusively as a fatigue phenomenon. There seems to be no controversy as to the existence of such peripheral fatigue, or as to its general chemical nature. What presents the fundamental difficulty in all fatigue studies is the determining by means of crucial experimentation the seat of fatigue in a given condition of decrement in efficiency of muscular, or even mental, work. Because even in cases of fatigue due to mental work there appears to be a tendency to search for its immediate source in the concomitant muscular tension, as the quotation from Dresslar on page 8 would indicate.

We return now to the fundamental problem of central inhibition and to its possible mechanism.

The stumbling block is represented by a problem of more general character, i.e., the problem of energy. McDougall (72), who apparently was the first to attempt a systematic, or better, an all-inclusive theory of inhibition, postulates "the distinction of the potential and living energy within the nervous system at any moment," and he suggests "the name 'neurin' in place of the cumbrous phrase 'living nervous energy'" (p. 171). Furthermore, he considers "that inhibitory effects in the nervous system result from a process of competition between the inhibiting and the inhibited tract, the former draining to itself a part of the energy liberated in the latter" (p. 191). The present writer agrees with a number of McDougall's critics that his terminology is rather unfortunately chosen, that the word 'neurin' has precisely a definite chemical or even pharmacological slant and betrays the author's inclination toward a hormic psychology. It may have been more appropriate, if his concept was really intended to designate energy, and only that, to label it with letters, say l.n.e., as the physicists are using e.m.f. to designate the electromotive force. The hydro-dynamic concept is still more emphasized by the term drainage, which is deeply associated with the general notion of material displacement. In toto, the whole terminology betrays a great similarity with concepts such as the "elan vital," etc., and McDougall recognized that but was willing to accept the challenge. This, of course, explains to some extent the fact that the physiologists had little patience with the theory, and a number of psychologists were inclined to follow suit. The extensive note on "neurin" with which McDougall found necessary to supplement one of his articles published in *Mind* (72) a year after the word Drainage was coined, seems to justify this critical attitude, and has apparently not contributed much to the further popularity of the terminology in the scientific fields most concerned.

There is, however, a more serious factor than the terminology that has contributed to the opposition to the Drainage theory, and that is the purely negative character of the phenomenon as treated by McDougall in his early abundant publications on the subject. Drainage is always given as a factor determining inhibition, and the concept of facilitation is at the best only inferred. The fact that when one process occurs, another previously set in operation will cease or show some impairment, does not necessarily indicate a misplacement of energy. One may imagine the bell of the telephone operating on one circuit and the speech transmitter and the receiver on another. When the receiver is lifted, the bell circuit is interrupted independently whether the current is or is not to be used for speech transmission. The blocking of one path while the other is set in operation may be due to incompatibility of purpose rather than to a shortage in energy. It seems pertinent to state here that whatever other factors a complete list of determiners of attention would include, the genetic element can not be overlooked. If the span and direction of attention are at all dependent upon ontogenetic factors, then the limitations of the organic inventory on the effector side may be of determining influence upon attentional behavior. Just as there is a reason to assume the presence of a genetic component in the visual process due to which double images that are frequently bound to occur are automatically disregarded, so there appears sufficient reason to infer that the span of perception becomes adapted to some extent to the amount and capacity of the effectors available. If one plays the flute from a sheet that has also the lyrics of the song, he may learn to disregard the words because flute playing and singing are incompatible even if the "living nervous energy" were available for both purposes. Relaxation of the flexors at least in part must occur not for reasons of economy, viz., that the energy is needed for the contracting group but because complete co-contraction would cramp the limb and make the movement impossible. The simultaneous occurrence of inhibition in one part of the neuro-muscular apparatus while another is in a state of intensive activity may imply a different mechanism than drainage in terms of interception of energy directed toward another center.

From the article in *Brain* we notice that among other analogies used to picture the propagation of the nervous impulses McDougall also uses the common analogy of "the firing of a train of gunpowder" where the explosion of powder particles started at one end propagates itself from particle to particle along the fuse. But this analogy to our understanding has come to contradict the hypothesis of *flow*, or *displacement* of substance or energy from one region to another. In a fuse no energy is transmitted but a process is propagated, the necessary energy being available in latent form in each successive particle of the fuse. If this analogy be true then one spark could set off into activity any number of pathways provided they somehow converge. The size of the spark, or the voltage of the battery required is in no proportional relationship to the amount of destruction produced by the blast. Moreover, if at some place of its course an accessory fuse should be led from the original (Razran), the initiation of the explosion in the side branch will not produce any decrement in the activity in the original trunk nor will it improve in any way in speed or other qualities if the activity in the main trunk should be extinguished for some reason. But it is precisely that which McDougall is asserting. In a footnote in his article of 1929 he repeats his statement made with reference to the subject in 1903 of which we shall quote only the final sentence:

> "If then two or more such arcs are connected together in their central parts, that one which is the most intensely excited will become for the time being the path of lowest resistance for the escape of neurin to the motor neurones, and will therefore, tend to drain to itself and to discharge by way of its motor neurones the energy liberated in all others." (75, p. 232)

This statement appears so ambiguous that it may be understood in the sense that not only free energy liberated in the higher centers may alternatively flow into one or another path, depending on the conditions of resistance in either, but that energy *liberated in the path* of higher resistance will flow backwards, reversing the usual direction at the synapses in order to flow into the path of lower resistance. The fact that McDougall repeats his original statement twenty-six years later, by quoting his own writing, would indicate that he has well evaluated its formulation.

However, in a footnote two pages ahead, McDougall quotes from his 1906 article and apparently clears the ambiguity when he states:

> "If, in accordance with the evidence adduced in an earlier article of this series, we regard the neural system of the upper brain levels as so many organized channels by which a common store of free energy, contained in the mass of afferent neurones and maintained in them at a varying pressure or potential by the streams of energy flowing in from the sense organs [I should now add—and from the instinctive centers in the basal ganglia] is discharged in efferent channels; and if also we assume, as we have seen good reason to do, that during the excitement of any such channel its normal or resting resistance is lowered in proportion to the intensity of its excitement then it follows that the excitement of any one such system must tend to inhibit that of every other, and that the system which at any moment is rendered, by a constellation of favoring conditions, the path of lowest resistance from afferent to efferent side of the brain will divert to itself and transmit to its efferent neurons the energy which is liberated in the afferent neurons of any other system simultaneously excited and so will inhibit that system." (p. 234, 1906 (75))

An analysis of the situation would indicate that the gunpowder analogy is not compatible with the "river-bed" analogy of the same author and they seem to contradict each other. However, since the first decade of this century, when all the fundamental articles of McDougall on drainage were written, new physiological facts were discovered which again seemed to suggest some spatial rearrangement of energy and an incompatibility of activity in two different pathways due to rerouting of stimulation in the direction of the pathway of higher excitation. These facts refer to the form of facilitation in which afferent stimulation corresponding to one pathway may release or accelerate the activity in another pathway which appeared at the moment in a state of activity or greater readiness.

Facilitation, as such, has been established by Sherrington (102), and by Ukhtomsky (110) and his students by direct animal experimentation. It is only in the 1926 article, in a polemic with Dodge, whose experimental "evidence" hardly contributes any material to offset the drainage hypothesis (see also Fearing (40)) that McDougall indicates that his theory may be corroborated by the phenomena of facilitation as well.

> " . . . It is not necessary to assume that every neurone path is in relation of reciprocal inhibition with every other. Reenforcement is a fact, no less than inhibition; and in terms of the drainage hypothesis, the result of simultaneous excitations of different paths must depend upon the synaptic arrangements concerned; under one arrangement inhibition, under another reenforcement." (p. 372, 1926 (74))

But in making this statement McDougall does not refer directly to any of his own previous writings, nor does he say anything about the nature of this arrangement.

In the article of 1929 we read again:

> "It is of the essence of this view that inhibition in the central nervous system is always a complementary or collateral effect in one neurone system of a process of excitation in another neurone system; and it is implied that the excitation process of the inhibiting system gains in intensity (or volume of energy transmitted through its neurones) at the expense of the inhibited system." (p. 233–34, (75))

On the other hand it appears that precisely the phenomena of facilitation give an apparent new impetus to the drainage hypothesis. It seems that Dodge (27) is right when he assumes that there is more than one form of inhibition or as he puts it: "various fundamental conditions of decrement." And it seems that this would hold also for the phenomenon of facilitation. One of these forms of facilitation is that of Dominance, or better, that of the Dominant (die Dominante in German), a rather cumbersome term in the English translation. Razran, who appears to be an authority on contemporary Russian neurophysiology, gives the following formulation of the theory:

> "When two or more units, or groups of units, or organismic action are activated simultaneously or in close succession, the action in the more dominant units, or groups of units, will thereby become heightened, while that in the less dominant will thereby become lowered." (1930, p. 27 (97))

It seems pertinent to clarify the concept of facilitation, as far as the situation of interdependence is considered.

Suppose we have two stimuli X and Y with their "normal" responses X_1 and Y_1 respectively. Let us suppose also that there is no postural incompatibility between the two. If now both stimuli are given together or at short intervals, or if at least the responses are to run simultaneously or in near succession, we may consider at least eight hypothetical cases of interaction:

	Response X_1	*Response Y_1*	*Energy supply liberated by stimuli*	*Distribution of energy*	*Dominance of one path*
1	Diminished	Diminished	Insufficient	Even	Absent
2	Normal	Normal	Sufficient	Even	Absent
3	Normal	Inhibited	Insufficient	All into one pathway	Present
4	Normal	Diminished	Insufficient	Uneven	Present
5	Increased	Inhibited	Sufficient	All into one pathway	Present
6	Increased	Diminished	Sufficient	Uneven	Present
7	Increased	Normal	Excessive due to mutual reinforcement	Uneven	Present
8	Increased	Increased	Excessive due to mutual reinforcement	Even	Absent

Let us now assume that we have found two concomitant activities where postural incompatibility is apparently excluded. One of the most representative of this kind is Fearing's (37) research on sway or maintenance of static equilibrium while counting the clicks of a telegraph key given at irregular intervals. It seems that a standing posture, if the eyes are kept shut, is controlled predominatly by reflex responses to stimuli from the semicircular canals and by automatized responses to proprioceptive cues; however, when control of sway is voluntarily performed in consequence of an instruction, "try to stand as still as possible," the responses to proprioceptive cues become apparently predominant, and are due to determination, or *Aufgabe.*

It appears that this voluntary control of sway is accomplished not by inhibition as such, but predominantly by active stimulation of the postural muscles. If that is the case, then the auditory stimulus for the concomitant task of counting the taps (which is more than a "simple" distraction by noise) has apparently produced a facilitation of the reflexes and voluntary reponses to proprioceptive stimulation. We deal here apparently with one of the conditions given in our list above under the numbers 7 and 8. The fact of interdependence appears to be definitely proved, but is it "drainage"?

Still more effectively is this interdependence demonstrated in the discussed thesis of Misbach (79, 80), who has found that both the knee-jerk and the finger choice reaction are affected due to nearness in time of occurrence and both in the direction of facilitation. We deal here with case 8 of our list. From Misbach's findings we could assume that the patterns of nervous excitation have some quantitative character, that their areas of spread somehow overlap and that this overlapping has produced an overexcitation in both fields. Whatever the arguments against a quantum theory as applied to nervous action are, it seems that Sherrington, Fulton and others imply a quantitative characteristic of the nerve process when they speak of algebraic summation of stimulation in the half-centers (see p. 82).

At least two alternatives are possible to explain this apparent interdependence between two activities. The regions of excitation in the brain for the two responses are overlapping and therefore two nearly simultaneous stimuli could reinforce each other. The other alternative would be that since the facilitated responses are not exactly simultaneous but only nearly so, the motor process of responding to one stimulus, say the knee-jerk, may elicit an additional proprioceptive stimulus which would accelerate the other motor response, viz., the pressing of the reaction key, or vice versa. But then the higher efficiency of the first one still has to be accounted for. The decrease in sway in Fearing's experiment during the performance of an attentional task presents a similar problem, if we consider that decrease in sway is under the conditions of his experiment a consequence of more intensive and more watchful motor control.

Let us now recall Ukhtomsky's (110) experiments. Stimulation of a brain area usually evoking a flexion of the limb would not produce any more this response when the rectum of the animal was filled, but would evoke the act of defecation instead. The contention is that the interoceptive stimuli from the filled rectum were producing an excitation in the centers controlling the sphincter and other musculature concerned with the act of defecation. The stimulation, however, was not intense enough at the moment to produce the act. The added stimulation in another center was "drained" in the direction of the previously activated path, and this reinforced the effector side of the arc and precipitated the act. If we consider the fact of the flexion of the limb not occurring, the interdependence is very suggestive of an analogy involving a displacement of energy. A more striking example of such a situation especially interesting to the psychologist is given by an experiment reported in 1933 by Ajrapetianz and Balakshina again from Ukhtomsky's laboratory (4). The spinal cord of a cat is cut twice, once below the fourth, and again at the seventh, lumbar segments. By means of electrical stimulation of the isolated segment the so-called Sherrington reflexes can be evoked. However, if a few days after the operation the recuperated animal is presented with a running mouse or a barking dog, the response of the hind legs to electrical stimulation of the isolated segment of the cord in the form of the

Sherrington reflexes disappears. However, as soon as the mouse or the dog is removed, or the animal is given the possibility of grabbing the mouse, these reflexes reappear again. The significance of the experiment lies in the fact that the only neural connection between the isolated spinal segment and the cerebral region is maintained by way of the visceral nerves.

The same authors also found that if the animal (cat) is abundantly fed before the operation, then decerebrated and the same cord transections performed, stimulation of the segment may not elicit the typical Sherrington reflexes, but the act of vomiting.

Again we deal here not simply with an inhibition of one act during the occurrence of another, but with the facilitation of one activity due to the stimulation in another pathway, while the latter appears incapacitated for adequate action. Using our connotation given on page 90 response X_1 was facilitated by stimulus Y while inhibiting at the same time response Y_1. The quoted authors find that there is no reason to revive the theories of special "inhibitory centers." If such is the case then the most remote facilitatory or inhibitory relationship between functional units of the nervous system is again corroborated, and with them to a certain extent the speculations of McDougall and the postulates of Ukhtomsky. In the discussion of their work Ajrapetianz and Balakshina suggest that the principle of the Dominant does not always necessarily involve the inhibition of *all* other neural paths when one of them is in action, but that a state of a Dominant occurs (1) under conditions of repetitive stimulation of one group of centers, (2) and only then when a simultaneous inhibition of other centers has taken place. The authors consider that it is possible that such a relationship may not exist at a given moment between two certain centers and that, therefore, the phenomenon of dominance will not always be observed. It seems then that they consider dominance only a particular case of inhibition and do not exclude other possible forms of inhibitory processes.

We return now to our own experiments, with reference to the influence of a concomitant task upon an automatized performance such as the act of tapping.

(1) In the first series we observe a slight decrease in speed of

reversal of the movement when a concomitant task involving attention and memory was performed or even when the stimuli for such a task were expected, although they did not appear. In this case the dominance, if any, appears on the side of the higher centers, the activity of which was initiated from the moment when the instructions were imparted, i.e., before the tapping began.

(2) In the second series, the tapping stroke immediately before the concomitant action but after the stimulus for the latter was given, is performed at a faster rate than the penultimate stroke, provided the subject is not fatigued. In this case, the dominance appears on the side of the automatized act. The rivalry between a mental process (determination to respond to the concomitant stimulus) and an automatized motor act is overshadowed by the rivalry between the latter and the concomitant voluntary motor response. At this stage of the experiment the dominance appears on the side of the automatized act.

(3) However, as soon as the concomitant motor response sets in, the speed of the automatized act is greatly reduced, the pattern of action is frequently disorganized, or the activity is often blocked entirely although for relatively brief moments. The dominance again appears but on the side of the concomitant motor task.

We must then conclude that during the simultaneous performance of two tasks the dominance may repeatedly shift in quick succession from one neuromuscular pathway into the other, intermittently favoring one or the other. The improvement in efficiency of multiple activity due to practice may well consist in part in the acquisition of a more efficient control upon both activities or in the reduction of the rivalry between the various systems in action.

It appears pertinent to mention that Ukhtomsky considers William James and McDougall as his direct predecessors in the formulation of the fundamental principles of the theory of the Dominant, although neither of the two has presented the required experimental evidence. He considers that the operation of the principle must be explained by postulating a thoroughly interconnected nervous "network" in which definite regions alternate in function. The conditions under which the waves of excitation in such an uninterrupted "network" produce facilitation in one region and inhibition in the other become the fundamental problems for experimental investigation. It is evident that for the psychologist as well as for the physiologist it is not so much a problem whether the phenomenon of interdependence in terms of facilitation and inhibition exists, but under what conditions and for what kind of activities either of these forms of reciprocal relationship is more readily established.

In the case of our experiment the automatized act was not capable of establishing a state of dominance of sufficient strength to prevent some retardation in the case of superposition of an ideational task and to escape definite disorganization in most cases of superposition of a predominantly motor concomitant task. The automatized activity, which supposedly is to be performed with reduced participation of higher centers is affected unfavorably by cortical activities such as discrimination, or remembering but is affected favorably by the conditions prevailing in the nervous system immediately before or at the moment of initiation of another motor task, provided the organism is not fatigued, the latter apparently being one of the conditions under which a Dominant may be established.

It appears then that the presence of interdependence and the form of the same, depends upon (1) the brain levels involved (2) specific conditions aroused by the task, (3) amount of practice, (4) fatigue, and (5) individual differences, and possibly other specific factors.

It appears from our experiment that a more analytical approach to the process of response, such as distinguishing the rate of movement from the rate of adjustment (in our case the reversal) may show that the observed interdependence between two activities is often a complex interplay of a large number of their components which determine finally the character of the "whole." It seems that this experiment has demonstrated that (notwithstanding the great contributions of Gestalt psychology in the emphasis of the significance of the "totality" of behavior) the minute analysis of components can often give an understanding of relationships between two phenomena, which would not have been observed if the acts had been taken only as indivisible configurations.

IX. CONCLUSIONS

1. A method has been found to record by means of the oscillograph four different components of the process of tapping with an extended index finger, or with a tool when a tapping frame is used.

2. These components are: (1) the movement up, (2) the upper reversal, (3) the movement down, (4) and the bottom reversal.

3. The records show that between both progressions there is a definite stop, which in finger tapping lasts from two to four times as long as the progressions themselves. In other words, an act intended and introspectively perceived as consisting entirely or predominantly of motion, in fact represents a situation in which the movement is arrested from seventy to eighty percent of the time, while only twenty to thirty percent is occupied by the displacement of the finger.

4. The components of the movement in almost all cases consistently possess a greater degree of variability in the time values than the total stroke.

5. Experimental conditions have been found under which the process of tapping has been affected by the presence of a concomitant attentional task supplemented by the necessity of retention. It has been found that the effect is slight but reliable and involves to a greater degree the time for the reversals than the time for the total stroke.

6. It has been found that if a concomitant choice reaction to light involving an intensive motor activity is to be performed with the left hand while tapping at top speed with the right, the rate of tapping is markedly reduced in speed *between* the concomitant reactions, and to a still greater extent *during* the concomitant response itself.

7. In addition to a marked decrease in speed during such a concomitant motor response, the tapping pattern itself is frequently broken up in the form of relatively long or excessively short stops at

the reversals (syncopation), or in the form of blocks in which the subject "loses his bearings" as to the width of the movement, returning to the bottom before having reached the top, or vice versa. Some exceedingly large values for the progressions lead one to infer that at least in some cases the subject may have been performing the tapping movement, but at a more narrow width, without reaching the bars of the frame, and apparently without being aware of the disappearance of the tactual cues.

8. However, this disorganization of rate and pattern lasts in all cases less than a second, showing a recuperation frequently still within the span of time devoted to the concomitant motor response. It appears then, that some phases of the motor concomitant show greater incompatibility with the tapping process than others.

9. The rate of tapping during the intervals between the concomitant motor responses, after about five seconds, reaches a decrement equal to the decrement occurring after fifteen seconds under normal conditions of continuous work, and is, at the end of fifteen seconds, greater by seventy-five percent than the decrement after thirty seconds of continuous work, under normal conditions.

10. The postural position involving the clenching of a metal handle with the left hand increases the rate of tapping provided the handle is not operated. As soon as the operation of the latter takes place, as in concomitant response, the rate decreases below normal.

11. At the beginning of the series with a motor concomitant, the stroke immediately preceding or overlapping with the concomitant response, tends to be shorter in duration than its predecessor (penultimate stroke). This difference is more frequent, and on the average, greater, with the presumably non-fatigued subject, than at the end of the series.

12. The use of a tool does not change the rate of tapping as such, but greatly affects the ratio between the values for reversal time and progression time.

13. The weight of the tool does not prevent the downward movement from being definitely slower with the tool than without it.

14. The operation of some kind of a compensatory mechanism which keeps the rate for tool tapping within 10 per cent of that for finger tapping by curtailing the reversal time when the progression time increases is evident. The various alternatives as to the nature of such a mechanism are discussed.

15. The variability of the stroke measured in terms of $\frac{100\sigma}{\text{Av.}}$ is in all but two of the forty-five experimental series definitely less than the variability of any of the components. There is no consistency in the amount and direction of change in the relative variability of the values due to continuous work, neither in the relations among the components nor within the successively obtained values for the same component.

Marked increase in variability of one reversal may or may not coincide with a decrease in variability of the other reversal, and an increase of variability up to one hundred percent in the progressions may appear in a series where the variability of the stroke is markedly decreased (compare data in series A and D, table IX).

16. An attempt was made to use variability of performance after rest as a measure of work previously performed, or as a measure of the degree of recuperation (as suggested by Weinland). The findings indicate that no generalization can be made to that effect, and that the increase or decrease of variability may depend upon the particular component of the behavior pattern which is *predominantly* investigated. This also would indicate that if an attempt should be made to consider variability as a personality trait, it will always be a variability in a certain performance, which may or may not be correlated with variability in other forms of behavior.

17. The drainage theory of McDougall and the principle of the Dominant as formulated by Ukhtomsky have been evoked for the explanation of certain features of the obtained results and the qualifications stated under which they may offer a plausible explanation. It appears that fatigue may reduce the capacity of a pathway for dominance and render it more susceptible to inhibition ("drainage"?) by other newly excited action systems.

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APPENDIX

TABLE XA

Bottom reversal. Frequency istribution for all experiments
For details consult tables in correspondng chapters and table XB in Appendix

TIME IN MS.	ATTENTION-MEMORY CONCOMITANT								ATTENTION-MOTOR CONCOMITANT																				FATIGUE EXPERIMENT																				
	Table Ia					Table Ib			Table IIIb—recheck for posture and fatigue			Table IIIa	Table VII												Table VIII										Table IX										
													Period X				Period Y				Period Z																								
	N	A	B	C	D	N	A	C	A	B	C	N	A	B	C	D	A	B	C	D	A	B	C	D	N	A	B	C	D	E	F	G	H	I	N	A	B	C	D	E	F	G	H	I	J
mid-point																																													
1.5																											1	4	3		1	3						4	3		1	3			
4.5																										2	3	2	7	5	5	1	3	1		2	3	2	7	5	5	1	3	1	6
7.5												2														—	—	—	—	—	—	—	—	—		—	—	—	—	—	—	—	—	—	—
10.5												—	1													—	—	—	—	—	—	—	—	1		—	—	—	—	—	—	—	—	1	—
13.5								1				1	—												1	—	—	—	—	—	—	—	—	—		—	—	—	—	1	—	—	—	—	—
16.5								—				16	3						1		3				5	2	4	—	—	—	—	—	—	1	3	2	4	—	—	1	—	—	—	1	5
19.5								—				24	3				1	1	—		4				4	3	2	3	1	1	—	2	1	—	2	3	—	2	1	1	—	2	1	—	4
22.5								—				7	—				1	—	—		—				3	2	3	4	2	2	3	5	2	1	2	2	3	3	2	3	3	5	1	1	6
25.5						1		—				—	—				—	—	—		—				—	3	1	—	—	2	1	—	—	3	—	3	1	—	—	2	1	—	—	3	5
28.5						—		—		1		1	—				1	—	—	1	—				1	4	1	—	—	—	1	—	—	1	1	1	1	—	—	—	1	—	—	1	3
31.5		2				—	1	1	1	1		4	—				—	—	—	—	—	1			14	6	4	3	1	1	4	2	3	—	10	5	—	2	3	3	2	2	3	—	7
34.5		—				1	—	1	1	2		2	—				—	—	—	—	—	—	1		5	11	2	2	3	1	2	1	1	1	2	7	2	—	4	2	2	1	1	1	9
37.5		—	6			6	1	—	4	2	1	10	1				—	—	—	—	—	2	—	2	9	5	7	5	3	3	2	3	1	1	4	3	—	2	1	5	1	—	—	1	5
40.5	5	1	3	2	1	6	1	3	2	—	—	13	—				1	—	2	—	1	1	—	1	14	13	9	3	6	—	2	3	5	6	6	10	—	1	4	1	2	—	1	2	2
43.5	5	4	4	3	2	16	7	8	3	4	1	22	1	1	2		—	1	1	1	—	1	2	2	12	6	11	5	8	6	5	8	2	6	13	3	8	1	9	2	4	2	—	2	2
46.5	6	6	5	2	1	6	5	9	9	3	2	23	1	2	—		—	2	2	1	2	1	2	2	7	9	7	9	8	5	6	10	6	9	7	10	6	3	2	—	1	2	4	2	1
49.5	8	17	1	—	—	8	17	10	3	2	3	28	1	1	—		2	1	1	—	3	2	1	1	4	14	12	8	10	10	8	3	8	17	4	5	10	5	4	2	1	1	6	5	1
52.5	14	23	7	7	13	10	7	8	10	10	13	69	1	5	5	3	1	1	2	1	1	5	4	7	10	11	17	17	23	21	2	10	7	8	2	1	13	10	6	5	1	2	7	6	2
55.5	18	16	2	6	6	15	3	5	7	9	6	25	3	3	3	2	2	1	2	2	2	2	4	5	6	6	2	10	8	7	6	4	6	3	1		1	5	4	4	3	2	3	1	—
58.5	43	42	26	25	29	10	4	11	10	5	10	62	7	2	10	7	2	3	3	1	11	15	11	17	14	5	8	10	7	15	10	13	17	10			3	7	1	8	6	16	5	13	1
61.5	16	12	26	32	25	9	5	2	4	—	4	23	1	3	2	1	4	5	2	—	6	5	9	7	—	—	1	7	1	4	5	5	7	—				2		2	4	5	5	—	
64.5	38	15	36	47	55	8	12	7	4	3	7	26	4	2	1	6	3	2	1	4	13	8	13	10	1	5	4	5	3	5	14	5	6	3				4		2	8	4	3	2	
67.5	17	9	15	32	22	10	10	9	3	5	3	10	2	4	3	4	2	—	2	2	16	6	2	4		—	3	1	—	—	2	1	4	2				1			3	—	2	—	
70.5	32	27	17	30	23	11	15	12	3	5	1	14	3	4	—	3	—	2	2	1	9	8	7	4		1	1	5	5	3	3	2	3	6							1	—	1	4	
73.5	8	7	10	9	10	10	5	13	1	7	2	9	3	3	2	2	2	5	1	2	7	4	10	4			4	1	1	1	2	2	1	3								—		—	
76.5	14	22	10	11	10	10	20	6		2	1	2	2	—	1	2	6	—	4	2	3	5	7	5			—	4	1	1	1	3	2	2								—		—	
79.5	11	7	3	8	6	3	7	—			3	1	2	1	1	2	4	2	2	1	2	12	5	4			1	1		—	1	4	1	3								1		1	
82.5	2	6	2	1	6	1	7	2						1	—		—	2	—	—	2	4	5	4						1	4	1	2	—											
85.5		3		2	1	1	6	1							1		3	2	1	1	2	2	1	1							3	1	2	—											
88.5		—		1	1	2	2	—							—		1	1	2	1	—	1	1	5										—											
91.5		1				—	2	3							1		1	—	1	1	3		—	—										2											
94.5		—				—	1	1							—		1	1	1	1			1	2																					
97.5		4				1		3							—		—	2	1	2				—																					
100.5		1						2							1		1	1	—	—				1																					
103.5								3										—	—	—				1																					
106.5								3										1	—	2																									
109.5								1										—	—																										
112.5								3										—	—																										
115.5								—										2	1																										
118.5								1											—																										
121.5																			—																										
124.5																			2																										
N	237	225	173	218	211	145	138	129	65	61	57	394	39	32	33	32	39	38	37	27	90	85	86	89	110	108	108	109	101	94	93	92	90	90	57	57	55	54	51	49	49	49	46	48	59
Av.	62.8	63.4	62.6	65.2	65.0	58.9	65.7	66.3	53.6	57.1	58.9	50.1	54.4	61.9	62.8	64.6	67.2	72.2	69.9	72.3	62.8	65.9	66.4	65.8	41.6	42.5	45.4	48.9	45.8	49.9	52.2	50.7	53.8	52.4	38.4	36.2	41.9	44.3	35.5	40.3	45.0	46.6	48.7	49.2	28.5
SD	9.3	12.0	9.5	7.7	7.8	13.4	13.7	19.8	9.5	12.1	8.9	14.9	19.9	9.9	12.5	7.7	18.3	20.4	23.0	19.1	16.0	12.1	11.0	13.9	12.2	12.7	15.6	17.1	17.2	15.3	19.5	18.0	16.0	15.8	9.6	11.2	14.6	18.9	17.9	18.3	20.3	19.6	16.2	16.1	12.4
V	14.8	18.9	15.2	11.8	12.0	22.8	20.9	29.9	17.7	21.2	15.1	29.7	36.6	16.0	19.9	11.9	27.2	28.2	32.9	26 4	25.5	18.4	16.6	21.1	29.3	29.8	34.4	35.0	37.5	30.6	37.3	35.5	29.7	30.1	25.1	30.8	34.7	42.6	50.5	45.4	45.2	42.1	33.2	32.7	43.6

TABLE XIA

TABLE XIA

Upward progression. Frequen istribution for all experiments

For details consult tables in correspo g chapters and table XIB in Appendix

TIME IN MS.	ATTENTION-MEMORY CONCOMITANT								ATTENTION-MOTOR CONCOMITANT										
	Table Ia					Table Ib			Table IIIb—recheck for posture and fatigue			Table IIIa	Table VII						
													Period X				Period Y		
	N	A	B	C	D	N	A	C	A	B	C	N	A	B	C	D	A	B	C
mid-point																			
1.5																			
4.5																			
7.5	7	4		5	19			1	1			30							
10.5	24	32	2	20	26		1	3	6			49	1	1			1		
13.5	50	66	64	82	64	2	15	12	29	16		102	11	2	2	2	4	2	1
16.5	44	19	30	14	26	11	30	17	8	15	5	57	12	8	5	1	2	1	3
19.5	69	16	34	37	40	29	24	42	14	16	21	69	7	14	9	12	16	14	3
22.5	20	16	6	25	20	27	14	19	5	3	14	27	1	1	4	5	2	4	8
25.5	17	30	15	27	17	30	15	17	2	8	12	23	3	4	7	8	4	6	11
28.5	3	18	4	7	2	22	22	14		3	4	8	—	1	—	1	—	3	4
31.5	1	12	16	3	1	12	12	3			1	12	1	2	3	2	4	3	3
34.5		5	1	1		8	4	1			1	7	—		—	1	—	1	—
37.5		5	1			3	2					6	3		2		—	1	2
40.5		—				1	—								1		1	1	2
43.5		1					1								—		1		
46.5															—		—		
49.5															1		1		
52.5																	1		
55.5																	1		
58.5																	1		
61.5																			
64.5																			
67.5																			
70.5																			
73.5																			
76.5																			
N	235	224	173	221	215	145	140	129	65	61	58	390	39	33	34	32	39	36	37
Av.	17.3	19.2	18.8	17.6	16.1	24.7	22.4	20.9	15.3	18.6	22.3	17.1	18.9	20.0	23.9	22.5	25.2	23.7	25.6
SD	4.7	7.7	6.1	5.6	5.3	5.5	6.6	5.1	3.9	4.5	3.8	6.6	6.8	4.7	8.0	4.7	11.9	6.2	6.2
V	27.0	40.3	32.4	32.0	32.8	22.1	29.3	24.3	25.7	24.0	17.1	38.8	36.0	23.7	33.4	21.1	47.3	26.1	24.4

TIME IN MS.					FATIGUE EXPERIMENT																				
					Table VIII										Table IX										
	Period Z																								
		B	C	D	N	A	B	C	D	E	F	G	H	I	N	A	B	C	D	E	F	G	H	I	J
mid-point																									
1.5																									
4.5																									
7.5																									
10.5																									
13.5	5	6	2									1													1
16.5	1	13	10	4								—													—
19.5	1	28	32	25								—													—
22.5	9	17	13	17								—					1								—
25.5	3	12	16	18	12	9	8	3	6	2		—			16	5	11	4	7		1		2	1	—
28.5	3	6	5	8	16	19	20	4	11	4		1			5	13	14	4	4	2	4	6	4	3	1
31.5	6	3	8	14	25	26	30	27	24	16	20	17	10	10	6	10	8	15	13	8	12	15	9	12	2
34.5	1	1	—	2	16	18	16	27	7	10	15	17	17	7	10	10	6	7	2	9	5	1	4	2	5
37.5	—		1	1	18	20	8	24	17	24	15	12	21	16	8	11	2	8	6	13	7	3	7	7	10
40.5	—				11	7	6	10	11	10	12	12	6	13	7	4	3	4	2	1	3	5	1	6	12
43.5	—				4	6	6	3	8	10	14	7	10	14	1	2	2	1	4	5	4	2	4	3	10
46.5	1				4	3	6	4	7	10	7	6	9	15	2	2	3	4	3	3	4	6	4	4	10
49.5					2		3	1	2	1	1	5	7	4	—		2	1	2	1	1	—	3	2	—
52.5					2		3	2	1	3	—	3	7	2	1		2	2	1	3	—	2	5	1	4
55.5							1	1	1	2	2	3	1	4			1	1	1	2	1	2	1	4	1
58.5								1	1	—	—	2	1	2				1	1	—	—	2	1	2	1
61.5									1	1	1	1	1	1				1	1	1	1	—	1		1
64.5									2		—	2						1	2	1	—	2			1
67.5											4	1							2		4	1			
70.5											2										2	—			
73.5																						—			
76.5																						2			
N	90	86	87	89	110	108	107	107	99	93	93	90	90	88	56	57	55	54	51	49	49	49	46	47	59
Av.	0.2	21.3	22.4	24.4	34.4	33.8	34.7	36.1	37.1	38.7	40.5	40.3	40.8	42.0	33.2	33.5	33.5	37.3	38.8	39.8	41.0	41.7	39.9	39.9	42.2
SD	6.3	4.6	4.8	4.9	6.3	5.2	7.1	5.8	8.4	6.9	9.5	9.3	7.1	7.0	6.6	5.3	8.3	9.2	12.0	8.4	12.5	12.9	9.4	8.9	8.0
V	1.2	21.4	21.4	20.2	18.2	15.5	20.4	16.1	22.6	17.9	23.4	23.0	17.5	16.6	19.9	15.8	24.9	24.6	30.8	21.2	30.4	31.0	23.6	22.5	18.8

TABLE XIIA

TABLE XIIA

Top reversal. Frequency d tribution for all experiments

For details consult tables in correspon ing chapters and table XIIB in Appendix

TIME IN MS.	ATTENTION-MEMORY CONCOMITANT								ATTENTION-MOTOR CONCOMITANT															FATIGUE EXPERIMENT																				
	Table Ia					Table Ib			Table IIIb—recheck for posture and fatigue			Table IIIa	Table VII											Table VIII										Table IX										
													Period X				Period Y			Period Z																								
	N	A	B	C	D	N	A	C	A	B	C	N	A	B	C	D	A	B	C	A	B	C	D	N	A	B	C	D	E	F	G	H	I	N	A	B	C	D	E	F	G	H	I	R
mid-point																																												
1.5																																												1
4.5																																												—
7.5																																												—
10.5																				1															1									1
13.5						2						2							1	—											1				—									—
16.5						—						5							—	1			1		2			1			1				2						1			6
19.5						1						2					2		—	—			—	2	—	1	1	—			—			1	—						—			1
22.5						1						—		1			1		—	—		2	—	3	4	—	—	—			—			2	6						—			6
25.5				1		—						2	1	—		1	1	1	—	—		1	—	3	4	—	—	—			1	1		3	7						1			1
28.5			1	—		1		1				2	1	—		—	1	—	—	3		2	—	3	1	—	—	1			2	1		2	2			1			2			3
31.5	2		1	1		—	1	—				4	—	—		1	1	—	3	3		1	2	6	4	2	3	—	2	1	2	1	3	7	4	2	3	—	1	1	1		3	5
34.5	—	2	—	1		—	—	1				7	1	—		—	1	2	3	1	1	—	—	1	—	—	2	—	—	—	—	—	—	1	2	—	1	—	—	—	—		—	3
37.5	1	3	2	3	1	5	6	3		1		15	2	2		1	1	1	—	3	4	—	2	6	6	1	3	4	2	2	3	3	1	11	2	1	3	3	1	2	2		1	6
40.5	4	4	2	—	1	4	6	—		—	1	11	4	2	1	1	1	—	2	3	1	2	2	3	2	2	2	4	—	1	2	1	2	4	3	2	1	3	—	1	—		1	1
43.5	10	5	—	3	2	3	5	3		2	1	24	2	2	3	1	2	3	1	2	8	4	4	5	9	1	1	4	1	4	1	1	1	3	2	2	2	3	—	3	—		—	5
46.5	9	3	7	5	5	8	5	—	1	—	2	28	4	1	1	3	1	3	—	7	3	6	4	4	12	11	8	7	9	8	4	4	4	—	5	11	3	4	5	5	2	2	2	10
49.5	12	10	4	6	6	13	11	9	2	—	—	25	2	2	2	1	—	3	—	4	5	6	4	7	8	15	8	7	4	3	3	6	3	2	4	10	5	1	1	1	1	1	2	3
52.5	15	16	11	13	9	13	13	13	1	—	1	35	2	1	2	—	—	2	2	5	8	5	6	8	12	16	15	7	14	9	3	10	1	—	2	8	13	3	4	3	5	3	2	2
55.5	11	7	4	12	20	13	8	12	3	2	6	12	1	2	3	3	1	1	1	6	—	2	7	10	7	7	5	7	7	8	3	7	7	3	4	3	2	2	3	4	1	6	2	1
58.5	19	11	14	14	22	20	17	14	9	3	8	49	4	4	1	4	3	1	2	6	9	10	10	14	12	10	10	12	14	6	12	8	12	5	7	2	5	9	9	4	11	5	5	1
61.5	16	4	8	10	12	8	23	18	12	4	4	21	2	—	—	—	—	2	—	5	1	1	5	9	2	5	7	7	5	9	7	1	10	5	—	—	4	3	2	7	5	3	4	1
64.5	26	9	11	15	38	16	3	7	14	12	9	34	3	1	4	1	4	1	2	7	4	8	3	12	6	6	18	14	13	13	9	9	13	6	4	4	5	7	11	10	6	7	10	—
67.5	17	10	7	11	17	4	6	8	8	10	6	25	1	2	1	3	1	2	1	3	3	2	7	2	2	3	1	4	1	2	7	4	6	1		3	1	5	3	—	2	3	5	—
70.5	31	17	13	13	29	4	6	10	9	10	11	34	2	1	1	—	3	3	2	10	9	4	2	7	3	5	4	4	5	5	11	10	8	1		2	3	3	4	1	5	4	4	2
73.5	11	6	10	17	13	5	7	2	5	6	4	14	—	3	2	—	4	3	—	3	2	6	5	—	5	1	3	4	5	5	8	5	6			1	2	2	4	4	4	4	2	
76.5	16	13	22	19	15	7	3	1	1	1	2	7	—	2	4	4	—	1	1	1	9	4	5	4	—	3	5	5	—	2	1	5	2			1	1	1	—	1		3	2	
79.5	6	22	22	13	10	6	2	6		4	1	18	1	2	2	5	1	2	2	6	3	4	9	2	4	9	3	1	3	2	—	3	3			2			1	—		2	2	
82.5	9	24	14	10	5	2	7	2		4		8	3	—	3	1	1	—	1	5	2	4	—		2	4	1	—	1	3	—	1	1			—				2		1		
85.5	6	15	8	9	5	4	4	5		1		5	—	1	1	1	2	1	1	4	5	4	6		1	4	3	2	—	3	4	1	2			—								
88.5	5	14	5	5	2	5	—	6				3	1	—	—	—	3	—	2	1	3	2	—		—	2	1	—	1	2	1	—	—			1								
91.5	4	17	5	12	1	1	2	3					2	1	1	1	1	1	2		3	2	3		—		2	—	—	1	1	—	1											
94.5	3	9	1	12	1	—	1	2						2	1		1	2	2		1	1	—		1		1	2	—	1	—	1	1											
97.5	—	2	—	6		—	2	—						—			1	—	—		—	1	—				1	1	1	1	2	1	1											
100.5	1	1	1	4		—		—						—			1	—	1		2	1	3				—	1	5	1	3	1												
103.5		—		2		—		1						—			—	—	1			—					—	1	1	1		1												
106.5		1		2		1								1			—	1	—			2					1	1				3												
109.5		1		1													1	—	—																									
112.5																		1	—																									
115.5																		—	1																									
118.5																		—	1																									
121.5																		—*	—*																									
N	234	226	173	220	214	147	138	127	65	60	56	392	39	33	33	32	40	39	37	90	86	87	90	111	109	108	109	101	94	93	92	89	88	57	57	55	54	50	49	49	49	44	47	59
Av.	64.8	72.4	69.7	71.8	65.6	59.7	60.5	63.9	63.8	67.7	63.6	57.4	56.8	63.2	65.8	62.4	63.7	67.0	70.4	59.1	64.7	63.8	63.9	52.3	51.7	59.5	59.9	60.1	61.6	62.8	62.1	63.3	62.8	44.7	40.7	54.2	53.9	56.6	60.1	58.3	56.9	63.9	60.5	36.5
SD	13.1	16.2	13.6	16.4	10.1	15.2	13.9	14.1	6.2	9.2	8.3	14.5	17.3	19.2	15.0	16.8	23.9	25.2	29.7	17.0	16.8	18.8	16.6	14.6	15.7	14.0	15.2	15.5	15.1	14.8	17.4	16.7	12.7	14.2	14.8	11.8	11.0	11.3	9.7	11.5	13.4	9.2	11.9	14.4
V	20.2	22.3	19.6	22.9	15.5	25.5	23.0	22.0	9.7	13.5	13.1	25.2	30.4	30.4	22.8	26.9	37.6	37.7	42.2	28.8	26.0	29.5	26.0	27.9	30.4	23.4	25.4	25.9	24.5	23.6	28.4	26.5	20.2	31.8	36.5	21.8	20.5	19.9	16.1	19.8	23.5	14.3	19.7	39.5

* For lack of space, one item each was omitted from: Col. B, midpts. 127.5 and 1.39.5; Col. C, midpts. 124.5 an 129.5. These items are included in the computation.

1 6 ★

TABLE XIIIA

Downward progression. Frequency distribution for all experiments

For details consult tables in corresponding chapters and table XIIIB in Appendix

TIME IN MS.	ATTENTION-MEMORY CONCOMITANT								ATTENTION-MOTOR CONCOMITANT											
	Table Ia					Table Ib			Table IIIb—recheck for posture and fatigue			Table IIIa	Table VII							
													Period X				Period Y			
	N	A	B	C	D	N	A	C	A	B	C	N	A	B	C	D	A	B	C	D
mid-point																				
1.5																				
4.5																				
7.5					20		1		1			23								
10.5	9	8		11	14		6	3	6			30	1				1			
13.5	74	77	49	87	66	40	32	23	32	1	1	149	7	8	2	1	4	5	2	1
16.5	61	38	37	39	34	17	32	25	10	29	3	54	11	7	4	4	9	8	1	4
19.5	81	49	44	41	75	26	31	39	13	20	29	83	14	8	17	15	13	7	14	9
22.5	8	8	14	11	3	34	10	29	3	9	16	27	2	2	3	4	2	5	5	4
25.5	1	21	18	23	2	16	6	7		2	5	17	2	7	1	5	3	8	7	4
28.5		14	5	2		12	21	2			1	1	—	—	1	1	2	—	1	3
31.5		9	5	2			1				1	3	2	—	5	1	1	3	2	1
34.5				3							1			1	—		1	—	1	—
37.5															2		1	—	1	2
40.5																	—	—	—	
43.5																	—	2	—	
46.5																	1		1	
49.5																				
52.5																				
55.5																				
58.5																				
61.5																				
64.5																				
67.5																				
70.5																				
73.5																				
76.5																				
79.5																				
82.5																				
N	234	224	172	219	214	145	140	128	65	61	57	387	39	33	35	31	38	38	35	28
Av.	16.6	18.3	18.6	17.3	15.6	19.6	18.9	18.8	15.2	18.6	21.2	16.0	18.4	19.3	22.2	20.9	20.9	22.0	23.4	22.8
SD	3.0	5.5	4.7	4.9	4.0	4.9	5.5	3.9	3.3	2.6	3.4	4.6	4.4	5.1	6.2	3.8	7.1	7.1	6.6	5.9
V	18.1	29.8	25.5	28.6	25.8	24.9	28.9	20.6	21.5	13.9	16.1	28.5	24.1	26.3	28.1	18.2	34.1	32.4	28.2	25.9

TIME IN MS.					FATIGUE EXPERIMENT																				
					Table VIII										Table IX										
	Period Z																								
	A	B	C	D	N	A	B	C	D	E	F	G	H	I	N	A	B	C	D	E	F	G	H	I	J
mid-point																									
1.5																									
4.5																									
7.5																									
10.5	17	6	4																						
13.5	26	19	13	8																					
16.5	25	45	47	42																					
19.5	4	7	9	15	1	1											4								
22.5	14	2	6	17	11	2	10	1	6				1		11		11	2	5			10	7	1	
25.5	1	1	4	5	17	15	19	10	7	4	3	2	6	1	9	6	11	10	6	2	2	3	6	5	
28.5	1	6	2	3	37	43	35	28	17	18	21	21	8	10	18	24	8	11	10	9	11	11	4	12	10
31.5	1		1		14	16	9	21	14	15	10	10	18	15	6	13	4	5	2	6	7	2	2	4	10
34.5					9	12	10	14	21	16	22	15	16	21	3	8	3	1	4	9	7	2	5	5	15
37.5					10	10	11	9	8	10	5	11	11	11	5	4	7	6	5	3	1	2	2	5	9
40.5					6	4	7	14	6	9	10	3	7	15	1	—	5	8	4	4	5	2	5	6	2
43.5					2	3	4	6	8	8	8	7	7	3	2	2	1	5	2	3	3	4	6	2	8
46.5					1	3	1	4	4	3	4	8	7	6	—			4	3	3	4	2	5	2	—
49.5					2		1	1	7	4	3	3	4	2	2			1	7	4	3	2	—	2	2
52.5								1	1	1	1	2	2	1				1	1	1	1	—	2	1	1
55.5									—	1	2	2	1	1					—	1	2	1	1	1	
58.5									—	2	1	2	1	1					—	2	1	2			
61.5									2		1	1							2	—	1	1			
64.5												—								1		—			
67.5												1								1		1			
70.5												2										2			
73.5												—										—			
76.5												—										—			
79.5												1										1			
82.5																									
N	89	86	86	90	110	109	107	109	101	92	91	91	89	87	57	57	54	54	51	49	48	48	45	46	57
Av.	19.0	19.7	20.4	21.8	33.5	34.2	33.8	36.7	38.3	39.1	39.4	41.7	39.7	39.8	32.8	33.9	32.1	37.4	39.3	41.9	40.8	40.3	38.4	37.9	38.7
SD	4.5	4.1	3.9	3.6	6.0	5.3	6.1	6.3	8.3	7.5	7.8	11.0	7.6	6.5	6.5	4.0	6.8	8.0	10.4	10.4	9.4	15.1	9.6	8.0	5.9
V	23.7	20.9	19.3	16.6	17.9	15.6	17.9	17.1	21.7	19.2	19.9	26.5	19.1	16.3	19.9	11.9	21.0	21.3	26.6	24.7	23.2	37.4	34.9	21.1	15.2

TABLE XIVA

Time per stroke. Frequency distribution for all experiments

For details consult tables in corresponding chapters and table XIVB in Appendix

TIME IN MS.	ATTENTION-MEMORY CONCOMITANT								ATTENTION-MOTOR CONCOMITANT											
	Table Ia					Table Ib			Table IIIb—recheck for posture and fatigue			Table IIIa	Table VII							
													Period X				Period Y			
	N	A	B	C	D	N	A	C	A	B	C	N	A	B	C	D	A	B	C	D
mid-point							*	*												
100.5							1	—												
103.5							—	—												
106.5							—	1				4								
109.5		1					—	1				3	1							
112.5		—					1	—				7	1							
115.5		1					1	1				7	1							
118.5		—			1	1	2	1		1		6	—				1			
121.5		—	1	1	—	2	2	—		—		25	1				—			
124.5	2	2	1	3	—	1	1	6		—		14	1				—			
127.5	6	2	2	4	9	2	2	2	3	—		29	2				1		1	1
130.5	6	1	3	3	4	3	3	1	3	—		15	1	3	1		—		—	—
133.5	6	4	3	3	6	3	2	4	3	2		32	2	—	—		—		—	—
136.5	14	3	3	1	9	6	3	4	11	—		26	1	2	—	2	—		2	—
139.5	12	6	6	5	9	11	5	1	5	4	1	42	2	2	1	1	1		—	—
142.5	10	5	4	5	5	3	4	—	4	3	2	34	2	—	1	—	2		—	1
145.5	8	6	3	8	10	8	1	1	8	6	2	37	1	2	1	—	2		—	1
148.5	7	7	8	4	10	5	4	2	3	4	2	26	4	—	1	—	2	2	1	—
151.5	15	9	4	8	10	8	3	5	4	3	3	24	2	2	4	1	2	—	2	—
154.5	10	5	4	8	5	13	8	2	4	2	5	14	2	—	—	3	—	4	—	—
157.5	25	6	—	10	12	7	5	6	2	4	2	13	—	1	2	3	1	1	—	—
160.5	15	4	2	6	10	7	4	6	4	2	5	13	1	2	2	—	1	—	2	2
163.5	12	9	8	14	12	13	8	19	5	7	6	7	4	5	3	3	3	3	1	2
166.5	12	6	11	10	20	9	15	12	5	3	6	5	2	3	1	—	1	—	2	—
169.5	11	16	16	19	16	5	12	8	1	4	13	2	3	1	—	2	1	1	1	—
172.5	8	12	9	10	15	2	7	4		2	5	1	3	1	1	2	1	—	—	—
175.5	7	24	23	18	14	4	4	3		4	4	—	2	—	1	2	2	1	1	—
178.5	6	11	13	14	15	—	3	4		4	—	2		3	5	3	—	2	3	—
181.5	5	17	20	5	7	4	2	2		2	—	1		3	4	1	4	2	5	1
184.5	6	12	14	16	7	4	3	4		3	—	1		2	4	3	4	—	—	1
187.5	6	10	6	6	5	3	7	4		1	1			1	—	2	—	5	2	1
190.5	7	9	5	10	2	6	1	5							—	—	—	1	—	—
193.5	7	11	2	4	—	3	3	3							—	3	2	2	—	1
196.5	3	11	1	8	1	2	2	3							1	—	2	—	—	2
199.5	5	3	1	8	—	3	3	—							1	—	—	1	—	1
202.5	2	3		5	—	4	4	3							—	—	—	1	—	1
205.5	1	2		3	1	1	5	1							—	—	—	—	2	2
208.5	1	1		1		2	1	1							1	—	—	1	1	1
211.5	—	2		—		—	3	—								—	—	1	—	4
214.5	—	—		—		1	2	—								1	1	—	2	—
217.5	1	3		—		—	—	—									2	1	—	—
220.5	1			—		—	—	1									—	1	1	—
223.5				—		1	1	1									—	1	—	—
226.5				1				—									—	1	1	—
229.5								1									—	—	—	—
232.5								1									—	—	—	1
235.5								3									—	1	—	—
238.5								—									1	—	—	—
241.5								1									—	—	—	1
244.5																	—	1	—	—
247.5																	1		1	—
250.5																	—		—	1
253.5																	—		1	—
256.5																	1		—	—
259.5																			—	—
262.5																			1	—
265.5																				—
268.5																				—
271.5																				—
274.5																				—
277.5																				1
N	237	224	173	221	215	147	139	129	65	61	57	390	39	33	35	32	39	34	33	26
Av.	161.0	171.9	168.3	169.9	160.5	161.9	166.1	167.1	147.0	159.7	163.0	139.5	149.0	160.9	169.0	170.8	176.0	186.3	184.6	195.1
SD	20.2	20.4	17.1	20.0	17.2	21.9	25.4	27.5	11.8	15.4	9.9	13.8	18.6	17.3	17.8	17.8	30.9	25.7	30.3	33.6
V	12.5	11.9	10.2	11.8	10.4	13.5	15.3	16.4	8.1	9.7	6.1	9.9	12.5	10.8	10.5	10.4	17.6	13.8	16.4	17.2

TIME IN MS.					FATIGUE EXPERIMENT																				
					Table VIII										Table IX										
	Period Z																								
	A	B	C	D	N	A	B	C	D	E	F	G	H	I	N	A	B	C	D	E	F	G	H	I	J
100.5																1									
103.5																—									
106.5																—									
109.5																—									1
112.5																—									—
115.5	1															—									2
118.5	—															1									—
121.5	1					1										2									—
124.5	1				1	3									2	2									6
127.5	3				2	6									3	7									5
130.5	1				2	2		1							3	4	1	1							2
133.5	2		1		5	2		—							9	5	2	—							2
136.5	3		1	2	4	—		—	2			1			5	2	1	—	2						2
139.5	3	2	1	—	8	3		—	1			—			8	3	1	—	1						4
142.5	2	2	1	—	4	3		2	1			—			2	2	1	—	1						4
145.5	6	—	3	—	5	8	4	—	—	1		—			2	6	4	—	—	1					5
148.5	5	5	1	1	5	8	4	1	2	—		—			—	1	5	1	3	1					2
151.5	5	8	5	3	4	10	4	2	2	1		—	2		—	6	4	3	4	3			1		2
154.5	5	1	3	4	7	8	14	9	3	—		2	—		4	4	8	3	3	—		2	—		3
157.5	8	7	4	4	5	6	9	4	3	—		—	—		3	1	6	1	2	—	1	—	—		4
160.5	5	2	3	7	6	7	8	6	2	2	1	—	1	1	1	2	1	5	1	1	3	—	3	1	3
163.5	3	4	2	5	4	4	9	8	5	3	2	—	3	1	4	—	3	6	3	1	1	2	3	2	3
166.5	5	7	5	6	5	4	6	10	5	4	6	—	1	1	1	3	2	4	4	3	2	—	1	—	3
169.5	3	6	12	9	5	3	5	6	8	4	2	1	4	1	3	2	—	7	4	2	1	8	2	—	1
172.5	5	8	6	4	9	—	4	1	5	5	4	3	4	4	2	—	4	2	3	3	2	2	1	3	1
175.5	5	7	4	6	5	1	6	6	8	12	3	10	2	5	3	1	4	6	2	5	3	5	1	3	2
178.5	6	5	5	8	5	3	3	5	9	2	3	6	3	3	—	—	3	2	6	3	3	1	—	3	—
181.5	2	3	4	5	9	4	3	8	8	10	9	7	4	11	1	1	1	3	4	5	5	2	2	10	—
184.5	4	4	3	5	3	6	3	2	7	8	7	5	5	2		—	1	1	3	5	5	3	2	4	—
187.5	1	7	7	6	3	5	1	4	5	6	7	17	5	7		1	1	2	1	3	5	8	3	1	2
190.5	—	5	8	3	2	3	3	1	3	7	7	3	7	10			—	1	1	4	6	3	3	6	
193.5	—	1	5	1	1	4	3	1	5	5	8	5	5	11			—	—	2	1	3	1	5	4	
196.5	2	—	2	3	—	—	4	4	3	4	4	4	8	4			1	2	1	1	—	3	6	1	
199.5	—	—	—	3	—	—	1	5	1	5	7	7	8	4				1		3	3	4	3	2	
202.5	2	1	1	1	—	2	4	4	—	2	1	2	3	4				2		1	1	2	1	1	
205.5	1	1		3	—	2	1	5	2	1	2	—	3	1				1		1	—	—	2	1	
208.5				—	1		1	—	1	—	1	—	4	3						—	1	—	2	2	
211.5				1	—		2	1	1	1	3	4	2	—						1	2	2	2	—	
214.5					—		3	3	—	—	1	1	—	1						—	1		—	—	
217.5					1		2	—	—	1	—	1	3	2						1			2	1	
220.5							—	1	—	1	—	—	2	1									1		
223.5							1	2	2	—	2	—	—	—											
226.5								2	1	—	2	1	1	2											
229.5								4	—	1	2	1	—	1											
232.5								—	1	1	—	1	—	1											
235.5								1	1	3	3	2	—	1											
238.5									1	—	1	2	2	2											
241.5									2	1	1	1	—	—											
244.5									1	1	1	1	—	1											
247.5											1	1	—	—											
250.5											—	2	2	1											
253.5											1	—	1	—											
256.5												1	2	—											
259.5													1	1											
262.5													1												
265.5													—												
268.5													—												
271.5													1												
274.5																									
277.5																									
N	90	86	87	90	111	108	108	109	101	92	92	92	90	87	56	57	54	54	51	49	48	48	46	45	59
Av.	160.0	169.6	172.1	174.4	160.9	160.0	172.3	180.4	180.6	186.9	193.9	194.3	197.4	194.8	147.0	143.5	159.1	171.2	167.8	180.0	184.4	183.1	189.1	185.8	146.0
SD	19.2	14.9	15.9	15.6	18.9	20.8	20.0	23.2	22.5	19.3	21.3	22.7	25.3	19.7	16.0	16.9	14.6	15.1	[or]	15.8	13.6	13.6	17.2	11.9	17.9
V	12.0	8.8	9.2	8.9	11.7	13.0	11.6	12.9	12.5	10.3	11.0	11.7	12.8	10.1	10.9	11.8	9.2	8.8		8.8	7.4	7.4	9.1	6.4	12.3

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* For lack of space, one item each was omitted from: Col. A, midpt. 91.5; Col. C, midpt. 97.5. These items were included in the computation.

TABLE Xb

Bottom reversal (supplement to table Xa, pages 106–7). List of measures (in terms of the midpoint of the class interval) whose deviation from the arithmetical mean is greater than 3 sigma and which were omitted from the frequency distribution in the calculation of the arithmetical mean and the standard deviation

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
Ia	A		19.5	1
	C		34.5	3
			31.5	
			22.5	
	D		34.5	4
			34.5	
			31.5	
			19.5	
Ib	N	124.5	13.5	2
	A	109.5	16.5	2
IIIb	C		25.5	1
VIIa	B		19.5	1
	C		13.5	2
			13.5	
VIIb	A	133.5		1
	B	241.5		2
		292.5		
	C	214.5		1
	D	223.5		2
		310.5		
VIIc	B		19.5	1
	C	109.5		1
	D		16.5	1
VIII	N		4.5	1
	A	109.5		1
IX	A	109.5		1
				Sum = 28

TABLE XIb

Upward progression (supplement to table XIa, pages 108–9). List of measures (in terms of the midpoint of the class interval whose deviation from the arithmetical mean is greater than 3 sigma and which were omitted from the frequency distribution in the calculation of the arithmetical mean and the standard deviation

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
I	N	34.5		2
		37.5		
	A	46.5		2
		136.5		
Ib	N	46.5		2
		67.5		
IIIa	N	40.5		4
		43.5		
		43.5		
		46.5		
VIIa	C	58.5		1
VIIb	A	82.5		1
	B	76.5		4
		76.5		
		127.5		
		136.5		
	C	67.5		1
	D	178.5		1
VIIc	D	49.5		1
VIII	N	61.5		1
	A	55.5		1
	B	64.5		1
	C	61.5		2
		64.5		
	D	67.5		2
		67.5		
	E	64.5		1
	G	76.5		2
		76.5		
	I	184.5	13.5	2
IX	N	61.5		1
	A	55.5		1
	I	184.5		1
				Sum = 34

TABLE XIIB

Top reversal (supplement to table XIIa, pages 110–11). List of measures (in terms of the midpoint of the class interval) whose deviation from the arithmetical mean is greater than 3 sigma and which were omitted from the frequency distribution in the calculation of the arithmetical mean and the standard deviation

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
Ia	N	109.5		3
		109.5		
		112.5		
	C		16.5	1
	D	100.5		1
Ib	A	143.5	10.5	2
	C		16.5	2
			13.5	
IIIa	N	112.5	10.5	2
IIIb	B		28.5	1
	C		28.5	2
			10.5	
VIIa	C		16.5	2
			16.5	
VIIb	B	220.5		1
	C	244.5		1
VIII	H	118.5		1
	I	127.5	16.5	2
IX	A	94.5		1
	D		16.5	1
	H		28.5	2
			25.5	
	I	127.5		1
				Sum = 26

TABLE XIIIb

Downward progression (supplement to table XIIIa, pages 112–13). List of measures (in terms of the midpoint of the class interval) whose deviation from the arithmetical mean is greater than 3 sigma and which were omitted from the frequency distribution in the calculation of the arithmetical mean and the standard deviation

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
Ia	N	28.5		3
		28.5		
		52.5		
	A	37.5		2
		100.5		
	B	37.5		1
	C	37.5		2
		43.5		
	D	31.5		1
Ib	N	31.5		2
		34.5		
	C	34.5		1
IIIa	N	37.5		7
		37.5		
		40.5		
		43.5		
		43.5		
		49.5		
		52.5		
IIIb	C	40.5		1
VIIa	D	79.5		1
VIIb	A	49.5		2
		85.5		
	B	79.5		2
		127.5		
	C	55.5		3
		226.5		
		235.5		
	D	82.5		1
VIIc	A	37.5		1
	C	37.5		1
VIII	N	70.5		1
	B	61.5		1

Downward progression—Concluded

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
VIII—*Concluded*	E	67.5		2
		70.5		
	F	70.5		2
		211.5		
	G	100.5		1
	H	76.5		1
	I	67.5		3
		103.5		
		292.5		
IX	A	49.5		1
	B	61.5		1
	F	211.5		1
	G	100.5		1
	H	76.5		1
	I	103.5		2
		292.5		
	J	67.5		2
		82.5		
				Sum = 51

TABLE XIVB

Time per stroke (supplement to table XIVa, pages 114–15). List of measures (in terms of the midpoint of the class interval) whose deviation from the arithmetical mean is greater than 3 sigma and which were omitted from the frequency distribution in the calculation of the arithmetical mean and the standard deviation

NUMBER OF TABLE	COLUMN	ITEMS >Av. + 3 × SD.	ITEMS <Av. − 3 × SD.	TOTAL NUMBER OF ITEMS OMITTED
Ia	A	248.5		2
		257.5		
Ib	A	250.5		1
IIIa	N	193.5		4
		199.5		
		214.5		
		217.5		
IIIb	C		124.5	1
VIIb	A	292.5		1
	B	286.5		6
		286.5		
		289.5		
		319.5		
		328.5		
		526.5		
	C	301.5		5
		357.5		
		360.5		
		375.5		
		412.5		
	D	313.5		3
		346.5		
		442.5		
VIII	A	256.5		1
	E	256.5		2
		256.5		
	F	310.5		1
	I	265.5		3
		316.5		
		484.5		
IX	N	199.5		1
	A	256.5		1
	B	223.5		1
	F	310.5		1
	G	244.5		1
	I	259.5		3
		316.5		
		484.5		
				Sum = 38

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HEMISPHERIC CEREBRAL DOMINANCE AND HEMISPHERIC EQUIPOTENTIALITY

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HEMISPHERIC CEREBRAL DOMINANCE AND HEMISPHERIC EQUIPOTENTIALITY [1,2]

A STUDY OF THE EFFECTS OF UNILATERAL CORTICAL LESIONS ON HANDEDNESS, PATTERN VISION, AND INTELLIGENT BEHAVIOR IN THE ALBINO RAT

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The theory of cerebral dominance assumes that the functions of handedness, speech, reading, and even memory, volition, intelligence, and personality are activities of one cerebral hemisphere: the left in right-handed individuals and the right in left-handed individuals. This theory is not based on experimental evidence but is suggested by the preferential use of one hand and by sporadic clinical or neuropathological cases. It assumes that cerebral lesions in the dominant hemisphere result in various psychological disturbances, whereas lesions in the non-dominant hemisphere do not result in these disturbances.

Anatomical examinations of the two hemispheres of the brain, however, have revealed no structural superiority of one or the other of the cerebral hemispheres. Studies on weight, size, shape, and other characteristics of the hemispheres confirmed the view that they are equal. Inferring function from structure, it is logical to assume that the cerebral hemispheres were equipotential in function. The theory of cerebral dominance assumes that one hemisphere is subordinate, whereas the theory of hemispheric equipotentiality assumes that both cerebral hemispheres function equally. No direct experimental evidence has been

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[2] This study was assisted by a special grant to Dr. Norman R. F. Maier from The Horace H. Rackham and Mary A. Rackham Fund.

offered for either theory, yet each is proposed without reference to the other.

REVIEW OF LITERATURE

Evidence for the theory of cerebral dominance in man

1. As related to the motor function of handedness. The fact that most of the motor fibers controlling each hand originate in the opposite side of the brain led many to contend that the hemisphere controlling the preferred hand is the lead or dominant hemisphere. The earlier theories of the structural superiority of one cerebral hemisphere were not supported by the facts in subsequent investigations. This led to the belief that one hemisphere is *functionally* superior, but that the differences can not be recognized in structure (Parsons, 1924).

In an attempt to explain why one cerebral hemisphere is functionally superior for hand preference it has been suggested that there is an unequal supply of blood to the two hemispheres. This has been based on the asymmetrical origin of the right and left carotid arteries. Since the heart is on the left side of the body, it was believed that the aorta is in more direct communication with the left cerebral hemisphere than with the right. Parsons (1924) does not accept this view, yet Jordon (1922) and Travis (1931) uphold the belief. Peterson (1932) refutes the theory on the basis of experimentation with lower animals.

2. As related to disorders of speech. Accepting the view that an exhibition of a dominant hand in man indicates a dominant hemisphere for motor functions, theorists extended the doctrine to include the function of speech.

Of the earlier writers, Broca, Bouillaud, Wernicke, and others[3] localized speech in various areas of the left cerebral hemisphere (for right-handed persons). Marie, Jackson, and Von Monakow disagreed with the localization of speech functions as postulated by these authors, and stated that speech, reading, and writing are functions of the whole of one hemisphere, usually the left in right-handed persons and *vice versa.* Marie thought of the abnormalities as disorders of the intellect and not sensory as up-held by Wernicke (Meyer, 1905). Jackson believed that the defect is a defect in propositional thinking.

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[3] Reviewed by Meyer (1905), Head (1926), and Travis (1931).

The theory of cerebral dominance as related to speech had its true beginning with Hughlings Jackson. In 1868 Jackson pointed out (James Taylor, 1931) that there are various degrees of aphasias, and that aphasia, agraphia, and alexia are defects in symbolic thinking. The higher and more voluntary aspects of speech tend to suffer more than the lower or automotive. He alleged that even writing is affected, not as a separate "faculty," but as a part of the failure to propositionise in words.

Jackson formulated his concepts through studies on epileptics. He cited many cases which have a bearing on the later works of Travis and of Orton.

In one of numerous articles Jackson asserts (Taylor, 1931; p. 19):

> In all cases of convulsions beginning unilaterally, it is important to consider carefully the side of the body in which the fits begin. We find that when persistent loss of speech occurs with hemiplegia, the hemiplegia is nearly always of the right side. I have long observed of convulsions that when spasms begin on the right side there is a defect of speech more marked than when it begins on the left.

Henry Head (1926) a student and admirer of Jackson, studied many cases of aphasia following cerebral lesions during the war. After extensive study of a number of clinical cases, Head concluded (p. 478):

> 1. There are no 'centers' for speaking, reading, writing or other forms of behavior comprised in the normal use of language . . .
>
> 2. Disorders of speech and similar high-grade defects of function are produced by injury to a single hemisphere, which in strongly right-handed persons is usually the left. . . .

The most recent advocate of the theory of unilateral hemispheric dominance in relation to speech disorders is Travis. The following statement of his is based on the concept of the physiological gradient as postulated by Child (1924), and the works of Head (1926) and of Orton (1925). He states (1931; p. 33):

> I believe the dominant center to furnish an inhibitory directional control over the subjacent centers and in the case of the left over the right circumjacent center as follows: Whenever energy is released in a sense organ, it tends to travel to the center of greatest instability (the dominant center), thereby losing some of its value in terms of amount and immediacy of termination for less dominant centers. . . . Anything that increases dominance increases the inhibitory directional control, while anything that decreases dominance decreases such control.

Travis holds that the left hemisphere in right-handed persons is the dominant or lead gradient. He says (p. 34):

> Practically no experimental work has been done to determine how sharply the highest dominant gradient may be delimited. But the vast array of clinical material in the form of diseased and injured nervous systems points strongly in the direction of the left cerebral hemisphere of the strictly right-handed individual and the right cerebral hemisphere of the strictly left-handed individual as containing the lead gradient, the more so in relation to such nice activities as speaking, reading, or writing.

The experimental evidence presented in favor of the theory is indirect and the conclusions equivocal. The evidence has been summarized by Travis (1933) who concludes:

> It is conceivable that, as the final common motor pathways out of the brain are approached, the right and the left motor impulses in some way, possibly by the former's accepting the latter's rhythm, coalesce to result in the perfect synarchy which is essential for unified control. It is thought-provoking to view this process as a dynamic act of unification on the parts of the two hemispheres producing a oneness in their outputs. Such an idea may be significant for the problem of cerebral hemispheric dominance.

It is indeed thought-provoking, but hardly evidence for the theory of unilateral hemispheric dominance.

In a study of aphasia Weisenburg (1934; p. 32) also supports the theory of cerebral dominance, for he states:

> Analysis of the side where the lesions appear in relation to handedness shows that the dominance indicated by handedness is a criterion of the crucial hemisphere for speech in about 95 per cent of the cases.

3. As related to reading and other visual perceptive processes. Hinshelwood (1917) stated that a lesion in the left hemisphere in right-handed people, and the right in left-handed people causes word-blindness. This supports the theory of hemispheric dominance in relation to reading defects. Hinshelwood based his conclusions on one autopsy in which the lesion was found to be in the left angular gyrus.

The most recent theory which has had wide publicity and acceptance especially among clinicians is the theory of "strephosymbolia." Orton (1925) observed that retarded readers usually reverse letters and words in reading. He also observed that some of them are fluent mirror-writers and fluent mirror-readers. He ascribed this ability—or inability—not to a cortical lesion as Hinshelwood had done, but to a "lack of cerebral dominance." The following is a statement of his theory (1928; p. 1096):

> . . . . The first level serves to give awareness that a visual sensation comes from without and is not a recalled memory of things seen; in psychologic terms, this level furnishes the element of external awareness in sensation. This function, without much question, resides in the area striata or calcarine cortex of the occipital lobes. The second level, that of objective memories, serves as the storehouse for visual impressions of objects which have been seen. This function probably resides in the second type of occipital cortex which surrounds the calcarine or striate area. Up to this point the two hemispheres of the brain apparently work in unison to produce a single conscious impression; i.e., the messages relayed from the eyes to the two sides of the brain are fused so as to give only one impression. This is brought into relief by the fact that neither of these functions is entirely lost as a result of the destruction of either hemisphere; a bilateral lesion is required to suppress the function of either the first or the second visual platforms. At the third or associative level, however, destruction in one hemisphere may result in complete loss of the associative function, resulting in inability to read (acquired wordblindness), while destruction of exactly the same area in the opposite hemisphere will not give rise to any symptoms whatever. That hemisphere in which destruction produces loss of the associative function is called the dominant hemisphere, and may be either the left or the right, according to the side which habitually initiates the motor responses of the individual. In other words, it is obvious that the

> visual records of one side only are used in symbolic association and those of the other are elided or inactive in this process.
>
> Structurally, however, there is no such contrast between the two hemispheres. The nondominant associative area is as well developed in size and complexity as is the dominant, and current neurologic belief, (neurobiotaxis) would imply that this silent or inactive area must have been irradiated equally with the active to produce an equal growth. Such an irradiation, moreover, would presumably leave behind it some record in the cells of the nondominant side which one may call an engram. The engram in the nondominant side would be opposite in sign, however, from that of the dominant; i.e., it would form a mirrored or antitropic pattern. Under usual circumstances only one of these reciprocally paired engrams operates in association with the concept in reading, as is shown by the facts of acquired word blindness already cited, and its antitropic or mirrored mate is elided or remains inoperative. If, however, the physiologic habit of complete elision of these engrams of the nondominant hemisphere were not established, their persistence might readily serve to explain the failure to differentiate between p and q and between was and saw, and also to account for facility in mirror reading and mirror writing, and thus to explain those confusions of direction which have been extensively recorded in the literature and which as here described seemed to characterize all the cases of my own series. Since this conception of the disability as a physiologic variant differs so widely from the pathologic moment known to result in acquired word blindness, I have felt that the use of the term congenital word blindness was misleading and have offered the term strephosymbolia—twisted symbols—to demarcate better the series of cases showing this typical symptomatology.

It will be noted that Orton assumes three psychological functions, corresponding to three cortical areas. He places "word-blindness" in the third level, whereas Hinshelwood had placed it roughly in the second level. He assumes (1) mnemonic records of letters or words existent in the brain in both dextral and sinistral orientation; (2) cerebral dominance; (3) a reversal of the visual image in the nondominant hemisphere, which normally is suppressed or elided; and (4) a shift of dominance in reversals in reading. Dominance is assumed as a fact but the evidence for it, and an explanation of it, are not given.

Experimental psychologists have not taken cognizance of the theory, yet many educators, clinical psychologists, and medical practitioners have accepted the theory as a working postulate. A few reactions toward the theory as it concerns reading may be mentioned. Tinker (1933; p. 348) believes that

> . . . one is justified in inferring that both left-eyedness and tendencies to sinistral sequences in the perception of words and letters are due to cerebral dominance.

While discussing the tendency of some children to reverse letters and words, Dearborn states (1933; p. 273):

> Orton has also given what appears to be an adequate account of this aspect of the difficulty in terms of brain physiology.

After discussing word-blindness, Leo Mayer states (1933; p. 1154):

> The third and final important contribution to this subject was made by Dr. S. T. Orton.

Among those critical thinkers who question the theory is Gates who, after discussing Orton's theory, states (1933; p. 4):

> It is an explanation that does not explain . . . . it offers a purely imaginary picture of brain structures and relations.

In regard to visual perceptive processes other than reading the following may be mentioned. Bender (1933) studied disturbances in visuomotor Gestalt function in cases with organic brain disease. She inferred the locus of the cortical deterioration from the neurological symptoms, and asserts (p. 537):

> In every case but one the symptoms pointed to a lesion in the left hemisphere; the one exception was a right-sided lesion in a left-handed man. . . . Thus, in a general way, we may conclude that the area most probably involved in disturbances of the Visuomotor *Gestalt* function, . . . is that between the temporal, parietal and occipital lobes of the dominant hemisphere.

As in most studies of this nature, handedness was not tested before the lesion, and no autopsies were made.

Franz, alone, has attempted an experimental test of the theory of unilateral cerebral dominance. He remarks (1933; p. 874):

> As far as I have been able to determine, the hypothesis is based on two series of facts: the preferential use of the right or the left hand for many activities; and the neuropathological correlates of the aphasias.

After describing a number of experiments which presented learning material to the left half of both eyes or to the right half of both eyes, he concluded (1933; p. 875):

> . . . in visual apprehension and learning there is not that hemispheral dominance that has been supposed. Those who invoke the concept of unilateral cerebral dominance should not extend its application to all cerebral functionings without further experiment. Moreover, the exhibition of a definite handedness does not warrant the broad generalizations that have become current.

4. As related to intelligence and higher mental processes. In his studies on aphasia Head (1926) emphasized the view that speech could not be localized and that a disorder of speech following a cerebral lesion in the dominant hemisphere was a disorder in "propositional thinking."

Dandy has performed cerebral operations for brain tumors in humans and has observed the effects of these extirpations on intelligence. In his most recent study (1933) he reported three cases of extirpation of most of the right hemispheres with no apparent decrease in mentality. The operations resulted in left-sided hemiplegias but did not affect intelligence. Dandy, however, made no claims for cerebral dominance, for he did not mention the handedness of the individuals, nor did he have comparable cases with extirpations of the left hemisphere. It is implied by others, however, that the left hemisphere is functional in intelligence. Diven (1934) interprets these results as indicating that mental activity is localized in the left hemisphere for right-handed individuals.

5. Summary. The theory of cerebral dominance in man has been applied to (1) the motor function of handedness; (2) the function of speech; (3) the visual perceptive functions of reading and other processes not involving language; and (4) intelligence and higher mental processes. In clinical practice a test of the dominant hand is accepted as a test of the dominant hemisphere. Lesions in the contralateral hemisphere supposedly result in disturbances of any or all of these functions, whereas lesions in the homolateral hemisphere do not disturb these functions.

Experimental pathological studies on animals

Studies of psychological disorders following controlled cerebral lesions in animals have shown certain functions to be similar to those found in man. In the rat are found (1) the motor function of handedness, (2) the ability to discriminate visual patterns, and (3) the ability to relate isolated experiences without learning (intelligent behavior). The more important studies relating to our investigation on the rat are summarized below:

1. Studies on handedness in the rat. Yoshioka (1930), Tsai and Maurer (1930), and Peterson (1931) discovered that rats exhibit a definite hand preference. This function was tested by a feeding situation in which the rat was required to reach for food with one or the other of its hands. It was found in several experiments that the handedness of the rat was a consistent and permanent characteristic. Peterson's experiment (1934) showed that the feeding situation was a reliable test of handedness in the rat. The consistency could not be explained by chance or habit.

Peterson (1931, 1934) has further shown that the hand preference of the rat in the feeding situation is dependent upon intact motor and somaesthetic fields of the opposite cerebral hemisphere. Lesions within a localized area in the frontal region of the contralateral hemisphere led to a transfer in the preferential use of the hands, whereas similar lesions in the homolateral hemisphere did not influence handedness.

2. The visual function. Lashley, in extensive studies on brightness discrimination and on pattern vision in the rat, assumed hemispheric equipotentiality although he did not deal directly with the theory. Lashley was interested in studying the effects of symmetrically bilateral lesions on the retention of the habit of brightness discrimination and of pattern vision.

In his earlier studies on brightness discrimination Lashley (1926, 1929) stated that: (1) Bilateral destruction of the whole of the posterior third of the cortex, after the brightness discrimination habit had been established, resulted in a loss of the habit. The loss, as measured by relearning, "was closely proportional to the extent of the injury and independent of its locus within the occipital third of the cortex." (2) Cerebral lesions in the frontal and parietal areas of the cortex did not affect the formation of the brightness habit, nor did they affect the performance of the acquired habit. In his most recent study (1935) he retracted some of the earlier conclusions and asserted that the retention of brightness discrimination was dependent upon intact striate areas, although relearning was not influenced.

Studies of the effects of cortical destruction on the acquisition or retention of reaction to a visual pattern for the rat are of recent date, and have been performed mostly by Lashley. The development of his jumping apparatus (1930) was the starting point for these studies. Previous to the advent of this apparatus, discrimination of form and size was not adequately demonstrated in the rat. With this apparatus it was found that rats could readily learn to respond to complex visual forms. This led to studies of cerebral functioning in relation to the acquisition and retention of response to visual patterns.

Lashley's earlier studies (1931, 1932) indicated that the rats failed to acquire the habit of reaction to a visual pattern when the optic radiations were interrupted. Postoperative disturbances, however, did not follow lesions in other than the posterior third of the cortex. In a later study of bilateral cortical destruction in relation to the retention of pattern vision, Lashley (1934) felt justified in drawing the conclusions: (1) complete destruction of both striate areas permanently abolishes pattern vision; (2) the postoperative disturbance was not dependent upon the extent of cortical tissue destroyed outside of the striate area. It appears that vision is more or less localized in the striate area, although the evidence is not conclusive or applicable to all complexities of visual patterns. It is safe to say that detail vision is localized in the posterior cortex and probably specifically in the striate area. The localization is not as specific as that for the function of handedness, but could not be considered general.

3. The function of intelligence. Studies dealing with the intelligent functions in the rat have almost exclusively dealt with bilateral lesions. Lashley (1929) studied the effects of bilateral cortical lesions on the capacity to form maze habits and on the retention of these habits. He concluded that the capacity to learn and the capacity to retain is reduced in proportion to the extent of cortical injury and not upon its anatomical specialization in the cortex. Thus, for the maze habit, lesions in any portion of the cortex result in a similar decrease in capacity. This makes the maze habit a general function and not dependent specifically on one cortical area (as the motor area) or upon a less specific function (as that of the visual area).

Maier (1929) has demonstrated that rats can combine the essentials of two isolated experiences in such a way as to reach a goal, when the experiences were not contiguous in time or space. In his experiments he set up a number of problems which furnished (1) an exploratory experience, (2) a feeding experience, and (3) the relating of these two isolated experiences in order to reach the goal. He states (1931) that this behavior cannot be explained by the traditional laws of association (contiguity, recency, frequency, intensity, primacy, etc.) and has designated this ability as "reasoning."

In later experiments (1932) he has presented evidence showing that reasoning (as defined above) and learning are qualitatively different. Experiments on the function of the cerebrum in relation to reasoning indicated that the ability is decreased in proportion to the extent of the lesion, regardless of its locus.

The experiments on the effect of cerebral destructions on reasoning have dealt wholly with more or less symmetrically bilateral lesions. No effects of unilateral lesions have been studied. Again hemispheric equipotentiality was assumed.

Summary of literature

Among the psychological functions ascribed to the dominant hemisphere in man are found (1) the motor function of handedness, (2) the visual-perceptive functions and (3) the functions of intelligence. These three processes have also been demonstrated in the rat. Experimental-pathological studies have tacitly assumed that the rats' hemispheres are equipotential, and disturbances of the functions (with the exception of handedness) have been studied by means of controlled bilateral lesions. These studies have shown (1) that handedness is more or less specifically localized in the motor area; (2) that visuo-motor habits are less specific in localization but may be ascribed to the posterior third of the cortex or specifically to the striate area; and (3) that intelligence (learning and reasoning) are very general and not at all localized. Since the rat exhibits a definite hand preference, it is now possible to study whether or not, the exhibition of a dominant hand is an indication of a dominant hemisphere for other psychological processes as well.

THE PROBLEM AND METHODS OF EXPERIMENTATION

The present problem

The purpose of this investigation is to furnish experimental evidence in the case of the rat for one or the other of the opposing assumptions, hemispheric cerebral dominance and hemispheric equipotentiality. By studying the effects of unilateral cortical lesions on (1) handedness, (2) retention of reaction to a visual pattern, and (3) intelligent behavior, the factor of dominance can be tested in specific, partially specific, and general functions, respectively.

General method of experimentation

With apparatus to be described later, the rats were (1) tested for hand preference, (2) trained to give a selective response to a visual pattern, and (3) tested for intelligent behavior on Maier's reasoning test. When the tests and training were completed, the animals were subjected to cerebral insults. For one group of animals the lesions were on the hemisphere opposite to the preferred hand (contralateral hemisphere); and for the other group the lesions were on the hemisphere of the same side as the preferred hand (homolateral hemisphere).

After a ten day period allowed for recovery from the operations, the rats were given postoperative tests of handedness, of retention of the visual patterns, and of reasoning ability. Histological studies of the brains were made at the completion of the postoperative tests.

Subjects

Forty-eight adult albino rats were used in the experiments. Of the 48 animals, 6 did not survive the operations and 1 died before completion of the tests. Thus there are records on handedness for 41 rats. A number of animals would not jump to the visual forms and were therefore excluded from the pattern-vision experiment. Only 28 rats completed the pre- and postoperative tests for this aspect of the investigation. For reasoning or intelligent behavior there are data on 37 rats since 1 rat would not run and 3 died before the experiment was completed. Rats number 10, 14, and 40 were males while the others were all females.

Surgical and histological methods

Each animal was trephined in one or two places on only one side, and cortex was destroyed by means of thermocautery. When the postoperative tests were completed the animals were killed and the brains were removed. The brains were then fixed, and the lesions were reconstructed on brain charts according to the method described by Lashley (1929).

The tests used

1. The handedness test. A T-shaped metal feeding cup similar to a canary feeding dish (Peterson, 1931) was constructed for the handedness test. The base of the T, which was narrow so that the rat could not reach the food with his mouth or tongue, was inserted between the wires of the cage. In order to feed, the rat was forced to reach the food (oatmeal) with one of his paws.

The tests were given in a rectangular wire cage divided by a board into two corresponding cages with a feeding cup in each.

Very few of the rats spontaneously used their hands on the first day. They scratched at the cage, projected their tongues into the feeding cup, or finally left the cup entirely. Several rats were placed in the cages for eleven days before they began using their paws. After the first successful attempt, however, the rats usually went directly to the feeding cup and used one of their hands to bring up the food. In order to avoid a place association with the surroundings of the cage, the rat was tested sometimes in one cage and sometimes in the other. Each rat was tested over a period of four days, and the successful reaches for food with each hand were counted until a total of 75 trials had been recorded. The test was given before and after the operation.

2. The pattern vision test. A jumping apparatus similar to that described by Lashley (1930) was used to secure responses to the visual patterns. The apparatus was built according to specifications except that the stand from which the rats jumped was a 6-inch circular disc mounted on a ring stand and attached to a window sill. As in Lashley's experiment the rats jumped away from the light.

In training the animals, the stand was first placed near the screen and the rats were allowed to step through the openings to the platform in order to obtain food. The distance between the screen and the stand was gradually increased until the rats were jumping through the windows from a distance of 25 cm. Then two white cards, held in place by light springs, were placed in the two openings. As the rats jumped at either window, the card fell back easily on the feeding shelf. Some rats showed no hesitancy in jumping at the white cards, while others refused to jump. In the latter cases, the stand was brought close enough for the rats to push down the white card by stretching, and then step through the open window. Again the distance was gradually increased until the rats were jumping at the white cards from a distance of 25 cm. When a rat had learned to jump freely with two white cards exposed, one of the white cards was changed for a black one held firmly in place so that the animal could not get through that window. This was changed irregularly from one window to the other until the rat learned to give a positive response to the white card, regardless of its position. The time required for this procedure varied from one to twenty days for the different rats.

After an animal had learned to jump to a white card alternated irregularly with a black one, the following visual patterns were presented (figure 1). The rats were trained to respond positively to the normal F and negatively to the mirrored F.

At this point it was very common for the rats to establish a place association and to jump consistently either to the right or to the left window. If the rat jumped to the same window for all ten trials of one day, the place association was broken the next day by again presenting the animal with white and black cards—the black card being placed in the window toward which he had been jumping consistently, and the white card in the other window. Usually three to five trials of this were sufficient to break the place association. If not, the procedure was continued until the habit had been broken.

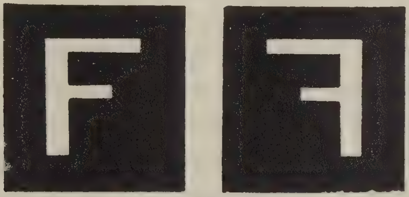

Fig. 1. The Visual Patterns Used in the Present Experiment
The figures are reduced to one-sixth the original size

Although some rats jumped readily as soon as they were placed on the stand, a few rats required urging by hitting or jarring the stand. The rats usually jumped more readily after learning had once begun. On the postoperative trials, the rats were much slower in jumping and required more urging. A few rats jumped for several hundred trials and then suddenly refused to jump. There seemed to be very little correlation between speed of response and rapidity of learning, for some rats needed urging yet learned rather rapidly; while other rats jumped very quickly, but required many more trials to learn.

Conditions of preoperative and postoperative learning. The rats were given 10 trials every day in jumping to the visual patterns. The position of the figures was changed irregularly. The training was continued until the rats had learned to a criterion of *50 errorless trials* over a period of five consecutive days.

After a ten-day rest, the animals were given a *preoperative retention test* which covered a period of five days. On the first two days 10 trials each were given as before, except that both windows were unlatched in order to test the rat's preference without the influence of relearning. On the last three days relearning was tested by repeating the conditions of the original learning. After this the operations were performed.

Following the ten days which were allowed for recovery from the operations the rats were given *postoperative tests.* These were the same as the preoperative tests except that relearning continued until one of the following two criteria was reached: (1) 30 errorless trials in three consecutive days (100 per cent success); or (2) 25 correct jumps out of 30 trials in three consecutive days (83 per cent success). The second criterion was used since two of the animals did not reach the first criterion of 100 per cent success. (Rat number 40 died after 280 trials; rat number 22 was discontinued after 330 trials.) Consequently we have 26 rats for the first and 28 for the second criterion.

3. Test of intelligent behavior—"reasoning." As previously described by Maier (1932, 1932a) the apparatus for this test consisted of three tables connected by elevated runways 1½ inches in width. The tables were of differing shapes, and were screened from the runways by large cardboards with holes large enough for the rats to pass through.

In the preliminary period the rats were allowed to explore the tables daily for one hour. At the end of the exploratory period of two weeks they were fed on any one of the tables. In the actual test situation the rats were again allowed to explore the tables for ten or fifteen minutes—experience 1. They were then carried to one of the tables where they were given a small amount of food. The experience of food on a specific table may be considered experience 2. The test consisted of placing the rat on one of the other two tables and observing whether it would proceed directly to the table on which it had experienced food. This test was repeated for eighteen days using different combinations of tables. In order to make better than a chance score the animal was required to relate experience 1 with experience 2, both of which had been presented in isolation. The rats were tested six days a week for three weeks (eighteen days) before operation, and similarly after operation.

RESULTS

A. The effect of contralateral cortical lesions on handedness

The validity of the handedness test. Peterson (1934) has contended that the feeding situation is an adequate test of a rat's hand preference. Our handedness data were also analyzed for the purpose of determining whether or not the rat's hand preference was the result of habit, or factors inherent in the test situation. For 24 of the rats a record was kept of the hand used and of the bodily position taken in relation to the feeding cup for two test periods of 75 trials (four days) each. This was done for (1) the original preoperative test, and (2) a second preoperative test six weeks later.

That the preference is not a matter of chance or habit is suggested from the following three lines of evidence:[4]

1. Some rats began with one hand but soon changed and used the other hand consistently after the first few reaches. Even in those cases which were consistent from the very first reach, the habit was probably not formed by so few trials, for other habits are not so readily established in the rat.

2. The position the rat took in relation to the feeding cup did not seem to determine the hand he used consistently, but rather, the hand preferred seemed to determine the position taken. The

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[4] Complete data is presented in the author's dissertation "The Effects of Unilateral Cortical Lesions on Handedness, Pattern Vision, and Reasoning in the Albino Rat," filed in the University of Michigan Library.

side of the cup opposite to the preferred hand apparently afforded much more efficient feeding. Of the animals whose handedness was changed after the operation none made a radical shift in position, but thereafter tended to feed from an inconvenient position. The position habit appeared to have been readily established because of the original hand preference.

TABLE 1

Showing consistency of handedness test

NUMBER OF RAT	FIRST TEST	SECOND TEST SIX WEEKS LATER
1	70 L— 5 R	74 L— 1 R
2	75 R— 0 L	75 R— 0 L
3	75 R— 0 L	64 R—11 L
4	75 R— 0 L	75 R— 0 L
5	75 R— 0 L	75 R— 0 L
6	75 R— 0 L	75 R— 0 L
7	45 L—30 R	66 L— 9 R
8	72 L— 3 R	73 L— 2 R
9	75 L— 0 R	75 L— 0 R
12	75 R— 0 L	75 R— 0 L
13	75 L— 0 R	75 L— 0 R
15	75 L— 0 R	75 L— 0 R
25	74 L— 1 R	75 L— 0 R
26	75 L— 0 R	75 L— 0 R
27	75 L— 0 R	75 L— 0 R
28	71 L— 4 R	75 L— 0 R
32	75 L— 0 R	70 L— 5 R
34	75 R— 0 L	75 R - 0 L
35	63 L—12 R	71 L— 4 R
37	75 L— 0 R	75 L— 0 R
38	75 R— 0 L	43 R—32 L
39	75 R— 0 L	75 R— 0 L
42	74 L— 1 R	75 L— 0 R
43	46 L—29 R	75 L— 0 R

3. The test was a reliable indication of the rats handedness. The results of the two preoperative tests are presented in table 1. It will be noted that for all rats the results are quite consistent. The most marked differences are found with rats numbers 38 and 43. Rat number 43, who was ambidextrous with a slight sinistral tendency on the first test, became purely left-handed on the second test. Rat number 38, who was purely right-handed

on the first test, became ambidextrous with a dextral tendency on the second test. Still less significant differences are found in the results on rats numbers 3 and 7, who, however, retained their ambidextrous classification. *In no case did a rat change from a predominantly left- to a predominantly right-handed tendency, or vice versa.* In other words, the handedness classification (right or left) as determined by the majority of reaches in 75 trials did not change for any rat. It was therefore unnecessary to test handedness over a longer period or with more trials. On the contrary, it was desirable to determine handedness with the least amount of practice in order to exclude any possible influence of practice.

The foregoing arguments and evidence tend strongly to suggest that for the rat testing hand preference by the feeding situation over a short period of time is adequate to determine handedness. The handedness of the rat as determined by the feeding situation appears, therefore, to be a stable and permanent characteristic.

Results of cerebral destructions. For all rats the post-operative handedness tests were begun ten days after the operation. The method of testing was the same as in the preoperative tests.

Table 2 presents the pre- and postoperative trials for handedness of 21 rats with contralateral lesions and 20 rats with homolateral lesions. Of the 21 animals with contralateral lesions, 1 rat (number 18) showed a partial change of handedness; 10 showed a complete change; while 10 showed no change. It should be noted that rat number 3 is not considered as having changed handedness. In the first place, as is shown in table 1, rat number 3 showed some ambidextrous tendency on the second preoperative test. Although on the first preoperative test it reached 75 times with its right hand, on the second preoperative test it reached 64 times with its right hand and 11 times with its left hand, indicating partial ambidexterity before the operation. Rat number 18 was considered to have shown a partial change of handedness. This case will be explained later.

The critical area necessary for handedness. An explanation of why some rats with contralateral lesions changed handedness while others did not is furnished in figure 2. It should be noted

TABLE 2

The effect of contralateral and homolateral cerebral lesions on handedness

NUMBER OF RAT	PERCENTAGE OF LESION	PREOPERATIVE HANDEDNESS	POSTOPERATIVE HANDEDNESS
Contralateral lesions			
Partial change of handedness			
18	15.9	L–75	L–40 R–35
Complete change of handedness			
23	21.3	L–75	R–75
24	21.5	R–67 L– 8	L–75
25	21.7	L–74 R– 1	R–75
28	23.0	L–71 R– 4	R–75
29	23.0	R–75	L–75
32	25.0	L–75	R–75
35	25.1	L–63 R–12	R–75
38	25.9	R–75	L–75
40	27.5	R–75	L–75
41	29.2	R–75	L–75
No change of handedness			
3	7.6	R–75	R–59 L–16
4	7.8	R–75	R–75
5	7.8	R–75	R–75
7	9.6	L–45 R–30	L–75
9	10.8	L–75	L–75
10	12.1	R–73 L– 2	R–75
12	12.3	R–75	R–75
13	13.2	L–75	L–75
16	14.7	R–75	R–75
20	17.0	R–75	R–75
Homolateral lesions—(no changes)			
1	5.5	L–70 R– 5	L–75
2	6.4	R–75	R–75
6	8.6	R–75	R–74 L– 1
8	10.5	L–72 R– 3	L–71 R– 4
11	12.3	L–75	L–75
14	13.5	R–75	R–75
15	13.8	L–75	L–55 died
17	14.9	R–75	R–75
19	16.3	L–75	L–75
21	17.8	R–75	R–75
22	18.7	R–75	R–75
26	21.9	L–75	L–75
27	22.8	L–75	L–75
30	23.3	R–75	R–75
31	23.6	L–75	L–75
33	25.0	L–75	L–75
34	25.1	R–75	R–75
36	25.4	L–75	L–75
37	25.6	L–75	L–75
39	26.5	R–75	R–75

that all lesions included a part of the posterior portion of the cortex. While some rats received lesions only in this region, others received lesions extending from the posterior to the anterior sections of the hemisphere.

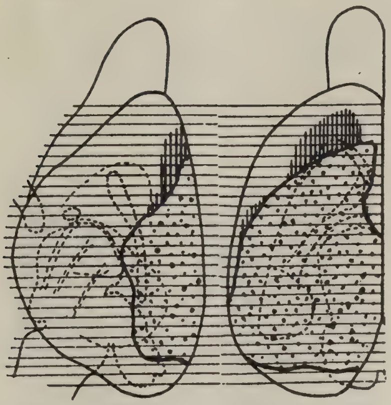

FIG. 2. The *stippled portion* represents a composite area of the cerebral destructions of those 10 rats with contralateral lesions who showed no change in handedness. The *hatched portion* represents the remaining common area for the 10 rats who showed complete changes of handedness. In this figure the composite of the 10 rats that did not change handedness is subtracted from the common area of those rats that changed handedness; thus leaving the *hatched portion; an area which, when extirpated, always resulted in a complete change of handedness.*

The stippled portion in figure 2 represents the combined area of the cerebral destructions of those 10 rats with contralateral lesions who showed no change of handedness. The hatched portion represents the remaining common area for the 10 rats who showed complete changes of handedness. In this figure the composite of the 10 rats that did not change handedness is subtracted from the common area of those rats that changed handedness; thus leaving the hatched portion, or the critical area, which, when extirpated, always resulted in a complete change of handedness. It should be pointed out that these areas are only approximations, but indicate that there is some localization for the use of the preferred hand.

The reader will note that rat number 18 had been eliminated from the previous considerations. This rat changed from a left-hand preference to ambidexterity after a contralateral lesion.

For this reason he could not be included in the group who showed complete change of handedness, nor in the group who showed no change of handedness. In figure 3 is shown the extent to which this animal's lesion overlaps the common area for the ten rats who showed complete changes of handedness. It appears that this one rat, whose lesion included approximately one-half of the "critical" area, showed an incomplete shift of handedness.

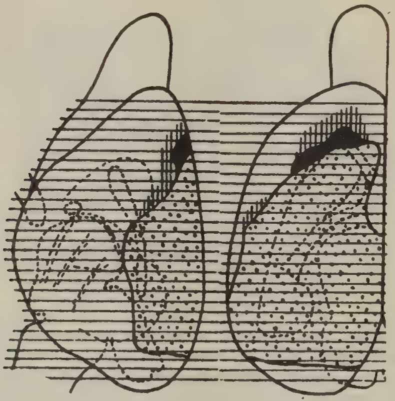

Fig. 3. The black portion represents the extent to which the lesion of rat 18 overlaps the common area of the 10 rats who showed complete changes in handedness (cf., fig. 2).

Unfortunately we have only one rat falling in this classification and therefore no conclusions can be made. Two explanations, however, seem plausible: (1) rat number 18 might have become ambidextrous without the operation as in the case of rat number 38 (table 1); and (2) extirpation of a portion of the critical area necessary for handedness might result in a partial shift of handedness; i.e., mass action within the critical area.

It is interesting to note the correspondence between the locus of Lashley's (1921) electrically stimulable points for arm and hand movement, and our critical area for handedness as shown in figure 4. The stimulable points, *k* (elbow flexed), *j* (forearm retracted), *i* (shoulder drawn forward), *l* (elbow extended), and *m* (wrist flexed) fall within or adjacent to our critical area for handedness. This correspondence leads to the conclusion that the critical area for handedness is approximately the same as the stimulable cortical points resulting in contralateral arm and hand movements.

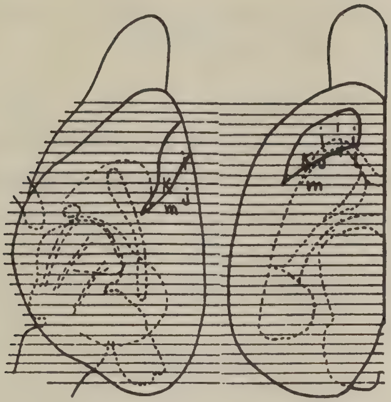

FIG. 4. THE CORRESPONDENCE OF THE CRITICAL AREA NECESSARY FOR CHANGE OF HANDEDNESS IN THE RAT, AND THE ELECTRICALLY STIMULABLE POINTS FOR ARM AND HAND MOVEMENT

The stimulable cortical points (after Lashley 1921) falling in or adjacent to the critical area are: *k*, elbow flexed; *j*, forearm retracted; *i*, shoulder drawn forward; *l*, elbow extended; and *m*, wrist flexed.

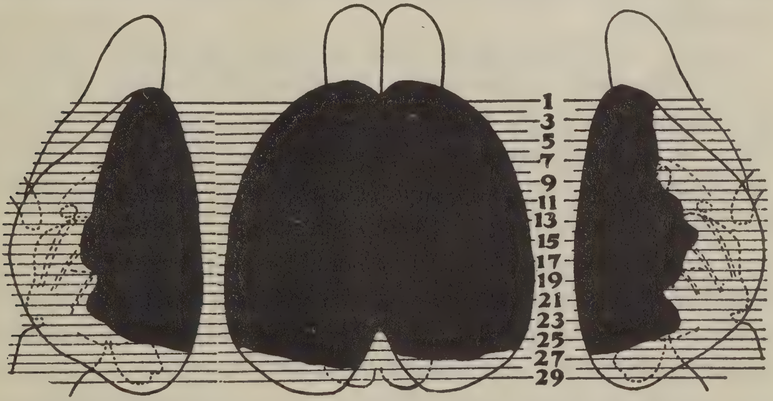

FIG. 5. SHOWING THE EXTENT AND LOCUS OF THE COMPOSITE AREAS OF THE HOMOLATERAL AND CONTRALATERAL LESIONS

The left half of the figure represents the composite area of the 21 rats receiving contralateral lesions; the right half represents the composite of the 20 rats with homolateral lesions. The average lesion for the contralateral group was 17.7 per cent; and for the homolateral lesions, 17.9 per cent.

Of the 20 rats with homolateral lesions, none showed a change of handedness. Figure 5 shows the similarity between the composite lesions in the homolateral and contralateral hemispheres. The average percentages of lesion for the two groups are approximately identical—17.7 per cent for the contralateral lesions and 17.9 per cent for the homolateral lesions.

Peterson (1934) has shown that after cerebral destructions in the critical area the rat changes its hand preference. Furthermore, he found that these changes resulted even when subcortical tissue was not invaded. In the present experiment the corpus collosum, the caudate nucleus, and the hippocampus were invaded in every rat that changed handedness. These were also invaded to a similar degree in many of the rats that did not change handedness.

Discussion

It seems apparent that the mechanism of handedness in the rat is dependent upon a definitely localized area in the contralateral hemisphere, and is not general as assumed by the theory of cerebral dominance. There still remains the possibility of mass action within the critical area as is suggested by the results on rat number 18.

Peterson (1934) has shown that the hand used after operation is used only from preference. Disabling the newly preferred hand with adhesive tape after a contralateral operation resulted in the use of the originally preferred hand only during the period of disability, but did not interfere with the postoperative preference after the tape was removed. He states (p. 16):

> The brain may be regarded as a necessary part of a mechanism which reveals handedness. Removing any part of the mechanism necessarily interferes with its functions, as would be obviously true if the hand itself were removed.

The present writer cannot completely agree with this statement on the basis of Peterson's own data. Removal of a hand permanently disables that hand. Removal of the cortex changes only the hand *preference*, not the *ability to use* that hand. Reeducation of the non-preferred hand over an extensive period of time may throw further light on the mechanism of handedness in the rat.

No adequate explanation can be given to account for hand preference in the rat. The fact that a localized cortical lesion changes this preference indicates that a critical area in one hemisphere is functional for this behavior pattern. It does not explain "dominance."

Conclusions for handedness

An analysis of the anatomical and behavioral data in terms of handedness leads to the following conclusions:

1. When a cortical lesion invaded a critical area of the motor and somaesthetic fields of the contralateral hemisphere, our rats changed handedness in the feeding situation.
2. When a lesion fell posterior to the critical area in the contralateral hemisphere, no change of handedness resulted.
3. Regardless of the extent or locus of the cortical destructions in the homolateral hemisphere within the limits of our lesions no change of handedness resulted for any rat.
4. The critical area for handedness corresponds approximately with the electrically stimulable cortical points for arm and hand movements.

B. *The effect of contralateral cortical lesions on pattern vision*

In so far as hemispheric dominance is based on a demonstration of a definitely preferred hand, it follows that the rat also has a "dominant" and a "non-dominant" hemisphere, since it exhibits distinct hand preference. In the foregoing sections evidence has been presented to show that for hand preference a certain critical area of the cortex is dominant over a corresponding area in the opposite hemisphere; and that removal of this critical area changes the dominance to the opposite hemisphere (as measured by hand preference). The present problem deals mainly with determining whether such a dominance is related to the perceptual function of pattern vision in the rat. In this connection we may raise several problems.

The first problem is concerned with the question of *whether there is a dominant hemisphere which controls learning, and which, when injured cortically, results in the loss of an acquired ability to* *respond to a visual pattern.* Table 3 presents the data on sixteen rats with contralateral lesions and twelve rats[5] with homolateral lesions. The numbers of the rats (column 1) and the percentages of lesions (column 2) correspond to the numbers and percentages of lesions given on the brain charts in the appendix. Columns 3 and 4 show respectively the number of trials and number of errors required for initial learning to the criterion of fifty consecutive errorless trials. Columns 5 and 6 refer similarly to trials and errors required for postoperative relearning (trials and errors preceding 30 errorless trials in three consecutive days). Column 7 shows postoperative errors to and *including* the 25 errorless jumps out of the 30 trials in three consecutive days (83 per cent criterion). Trials were eliminated here since by this criterion there was no differentiation between animals making no errors and those making up to five errors within the minimum 30 trials.

According to the theory of unilateral cerebral dominance, the group of rats with contralateral lesions should lose the habit of responding correctly to the visual patterns; whereas the group with homolateral lesions should tend to retain the ability. The data presented in table 3 do not uphold the theory, but rather indicate that the hemispheres are equipotential for pattern vision. This is shown from two differing modes of analyzing the data.

1. In considering the number of cases of each group which retained the habit, we shall arbitrarily consider 6 or less errors on

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[5] Of the 41 rats used for the handedness experiment, 13 did not complete the tests for pattern vision. These are: (1) Rats numbers 17, 26, 32, 38, and 39, who, on the preoperative tests, could not be trained to jump, or who refused to continue jumping after bumping their noses several times. (2) Rats numbers 33, 34, and 35, who, in 600 preoperative trials could not learn to the criterion of 50 errorless trials. (3) Rats numbers 31, 36, and 41, who refused to jump after the operations. These rats were also very poor jumpers before the operations. Rats numbers 31 and 36 received homolateral lesions; rat number 41, contralateral. (4) Rat number 15, who died before completion of the postoperative retention test. (5) Rat number 23, who did not complete postoperative relearning. Even in initial training, this animal had difficulty in jumping directly at the windows. After the operation this peculiarlity was so exaggerated that he could not relearn in 500 trials. With the results on these animals eliminated, we have 26 rats who relearned to 100 per cent perfection or 28 rats who relearned to at least 83 per cent perfection.

TABLE 3

The effect of contralateral and homolateral cerebral lesions on the retention of pattern vision

NUMBER OF RAT	PERCENTAGE OF LESION	INITIAL LEARNING		POSTOPERATIVE RETENTION		
				100 per cent criterion		83 per cent criterion
		Trials	Errors	Trials	Errors	Errors
Contralateral lesions						
3	7.6	200	63	0	0	0
4	7.8	365	123	20	5	5
5	7.8	299	80	20	9	9
7	9.6	149	29	16	3	3
9	10.8	235	58	52	16	16
10	12.1	104	42	18	3	3
12	12.3	190	58	61	21	21
13	13.2	230	72	25	5	5
16	14.7	122	49	0	0	0
18	15.9	160	77	277	120	120
20	17.0	76	29	120	51	51
24	21.5	189	47	128	47	39
25	21.7	133	44	89	33	30
28	23.0	125	51	159	35	10
29	23.0	146	29	97	36	36
40	27.5	197	79	*	*	52
Average.....	15.3	183	58.1	72	25.6	25
Homolateral lesions						
1	5.5	387	131	11	3	3
2	6.4	167	65	100	24	5
6	8.6	339	101	45	9	7
8	10.5	140	44	134	47	43
11	12.3	156	51	140	62	62
14	13.5	108	48	30	5	5
19	16.3	109	51	20	5	5
21	17.8	161	59	274	77	46
22	18.7	278	66	*	*	75
27	22.8	109	42	240	74	61
30	23.3	76	29	20	4	4
37	25.6	120	43	119	52	52
Average.....	15.1	179	60.8	103	32.9	30.6

* Did not learn to the criterion of 30 errorless trials (100 per cent).

the postoperative tests as indicating immediate retention since this was the highest error obtained on the pre-operative retention tests. Using the 100 per cent criterion, we note that 4 of the 11 rats of the homolateral group (36 per cent) retained the ability, while 6 of the 15 rats of the contralateral group (40 per cent) also retained the ability. Similarly, using the 83 per cent criterion, 5 of the 12 animals of the homolateral group (42 per cent) and 6 of the 16 animals of the contralateral group (37 per cent) retained the ability. It is also obvious that when we consider loss of the habit, there is a comparable number from each group which made more than 6 errors. By these measures, it may be concluded that the hemispheres are equipotential; i.e., cortical lesions produce similar disturbances of the habit of reaction to a visual pattern whether the lesion is in the homolateral or in the contralateral hemisphere.

2. Considering the average scores for the two groups, we find that there are but slight differences between these two in percentages of lesion and in initial learning scores. It should also be noted that the locus of lesions for both groups is very similar, as may be readily seen from the brain charts in the appendix. According to the theory of cerebral dominance there should be greater retention by the group with lesions in the homolateral hemisphere, but our results show, if anything, a slight trend in the opposite direction. The average postoperative retention scores for the contralateral group were: 72 trials and 25.6 errors for the 100 per cent criterion; and 25.0 errors for the 83 per cent criterion. For the homolateral group the corresponding scores were: 103 trials and 32.9 errors for the 100 per cent criterion; and 30.6 errors for the 83 per cent criterion. These scores indicate that the group with lesions in the homolateral hemisphere took more trials and made more errors than did the group with lesions in the contralateral hemisphere. This difference, of course, is probably due to chance factors and might be expected from the averages of small groups.

Thus by neither method of analysis do we find a greater loss from the contralateral lesions. Instead, the evidence points to hemispheric equipotentiality.

The second problem is related to the theory of "strephosymbolia." In this connection one may ask: *Is there any indication that after a posterior lesion in the contralateral hemisphere the animal will jump toward the reversed figure?*

Orton (1925) has postulated that when there is a conflict in cerebral dominance of a human the person tends to reverse words and letters in reading them. According to Orton, these symbols are "etched" normally in the contralateral and mirrored in the homolateral hemispheres in the third (visual associative) level. In the condition of lack of cerebral dominance the mirrored symbol in the homolateral hemisphere causes a reversed response.

According to this theory a child learning the word "no," for example, will have an engram of "no" in the contralateral hemisphere, and an engram of mirrored "no" in the homolateral hemisphere. In normal reading the contralateral hemisphere is functional, while the engram of mirrored "no" in the homolateral hemisphere is elided. In the event of a lesion or a decrease in the dominance of the contralateral hemisphere, there results a verbal motor response, "on," to the visual symbol "no."

In our experimental situation the rats were trained to give a positive motor reaction (jump) to the pattern F, and a negative motor reaction to its mirrored counterpart (mirrored F). If Orton's theory were applicable to the rodent, we should expect that a lesion in the contralateral hemisphere should decrease the dominance of that hemisphere and result in the functioning of the previously non-dominant hemisphere (the homolateral hemisphere). In this case we should expect the rat to respond positively to the mirrored F and negatively to the normal F. The criticism may be raised that we have no evidence for the third visual level (visual associative) in the rat, but such a criticism is readily answered by the fact that neither has the level been demonstrated for humans.

Table 4 presents the data on the effects of contralateral and homolateral lesions in producing a tendency to respond to reversed figures. In column 3 are presented the preoperative retention scores for the first two days of the test. The scores are given in terms of right minus wrong responses. The cards pre-

TABLE 4

The effect of contralateral and homolateral cerebral lesions on reversals

NUMBER OF RAT	PERCENTAGE OF LESION	PREOPERATIVE RETENTION TEST (R–W)	POSTOPERATIVE RETENTION TEST (R–W)
Contralateral lesions			
3	7.6	18	18
4	7.8	8	0
5	7.8	18	2
7	9.6	20	8
9	10.8	20	6
10	12.1	18	2
12	12.3	20	4
13	13.2	4	10
16	14.7	20	14
18	15.9	18	0
20	17.0	0	0
24	21.5	16	0
25	21.7	16	0
28	23.0	0	4
29	23.0	20	0
40	27.5	18	2
Average.........	15.3	14.6	4.4
Homolateral lesions			
1	5.5	2	2
2	6.4	16	8
6	8.6	20	0
8	10.5	8	0
11	12.3	12	0
14	13.5	20	0
19	16.3	20	14
21	17.8	20	0
22	18.7	12	0
27	22.8	16	−6
30	23.3	20	4
37	25.6	20	0
Average.........	15.1	15.5	1.8

sented were both unlatched so that no relearning could take place. In column 4 are given the postoperative retention scores for the same conditions after the operations.

The average percentage of lesion and the preoperative retention scores are approximately the same for the two groups. According to the theory we should expect minus scores from animals with contralateral lesions. This would be the case if the rats responded positively to the reversed pattern (mirrored F). The results did not show this to be so, for only one negative score occurred, and that was in the homolateral group. This is what would be expected on the basis of the preceding section which showed that the hemispheres of the rat are equipotential for pattern vision, whereas the theory of "strephosymbolia" is based on the assumption of hemispheric dominance.

We make no claims of analogy from rat to man. Our results are only applicable to the rat. Furthermore, it must be pointed out that this is not a conclusive test of Orton's theory. It is believed, however, that the theory of "strephosymbolia" is highly speculative, and before it can be accepted its existence should be supported by some evidence other than that derived from numerous assumptions (see p. 6).

A third problem raised is concerned with the *relation between the extent of cortical destruction and the degree of postoperative disturbance.*

An explanation for the disturbance of the habit of reaction to the visual pattern is found in the relation between extent of cortical lesion and degree of postoperative disturbance. Kirk has analyzed these data elsewhere. A correlation of extent of lesion with postoperative trials was +0.48 and with postoperative errors +0.52. An analysis of the data showed that the disturbance of the habit was not dependent upon (1) the extent of striate area destroyed; (2) the locus of striate area destroyed; or (3) the extent of posterior lesion. From these results it was concluded that:

1. The loss of the habit of reaction to a complex visual pattern in terms of relearning is closely proportional to the extent of the lesion and is not mainly dependent upon the destruction in the area striata or the posterior cortex.
2. The results may be interpreted as evidence for extra-striate functions in the discrimination of complex visual patterns.

Conclusions for Pattern Vision

1. Retention of a motor response to a visual pattern suffers the same loss from a homolateral lesion as from a contralateral lesion. There is no evidence for hemispheric cerebral dominance in the rat; rather, the hemispheres appear to be equipotential for this function.
2. A cerebral destruction in the contralateral hemisphere does not produce a positive response to a pattern which is the reverse of the one to which the response had been previously established.
3. In terms of relearning the loss of the habit of reaction to a difficult visual pattern is closely proportional to the extent of the lesion and is not entirely dependent upon the destruction in the area striata.

C. *The effect of contralateral cortical lesions on intelligent behavior*

Table 5 gives the pre- and postoperative scores on Maier's reasoning test for two groups of rats—a group with homolateral lesions and a group with contralateral lesions. Columns 3 and 4 present the preoperative scores in terms of the number of correct and incorrect first runs in the test period of eighteen days. Similarly the last two columns present the corresponding postoperative scores.

According to the theory of cerebral dominance as applied to intelligent behavior, one might expect a greater reduction in the scores of the group with contralateral lesions, than in the scores of the group with homolateral lesions. This was found not to be the case. Cerebral destructions were followed by an appreciable decrease in the postoperative scores in both groups of rats, but the scores for the two groups are strikingly similar. The average percentage of lesion for the contralateral group was 18.3; for the homolateral group 18.8. On the preoperative tests the groups were again homologous—the contralateral group scoring 75.5 per cent;[6] the homolateral group 77.7 per cent.[6] The post-

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[6] Calculated according to Maier (1932a): $\frac{\text{Right minus wrong}}{\text{Total}}$.

TABLE 5

The effect of contralateral and homolateral cerebral lesions on reasoning

NUMBER OF RAT	PERCENTAGE OF LESION	PREOPERATIVE SCORES		POSTOPERATIVE SCORES	
		Correct	Incorrect	Correct	Incorrect
Contralateral lesions					
3	7.6	16	2	15	3
4	7.8	14	4	13	5
5	7.8	14	4	15	3
7	9.6	17	1	16	2
10	12.1	17	1	18	0
12	12.3	15	3	16	2
16	14.7	14	4	17	1
18	15.9	17	1	15	3
20	17.0	17	1	18	0
23	21.3	18	0	15	3
24	21.5	17	1	15	3
25	21.7	16	2	14	4
28	23.0	16	2	14	4
29	23.0	15	3	17	1
32	25.0	17	1	11	7
35	25.1	14	4	11	7
38	25.9	16	2	8	10
40	27.5	15	3	11	7
41	29.2	15	3	12	6
Average....	18.3	15.8	2.2	14.3	3.7
		75.5%		58.9%	
Homolateral lesions					
1	5.5	14	4	15	3
2	6.4	17	1	12	6
6	8.6	17	1	14	4
8	10.5	15	3	15	3
11	12.3	15	3	16	2
14	13.5	15	3	18	0
17	14.9	16	2	17	1
19	16.3	17	1	15	3
21	17.8	15	3	14	4
22	18.7	14	4	10	8
26	21.9	14	4	10	8
27	22.8	18	0	14	4
30	23.3	17	1	16	2
33	25.0	16	2	13	5
34	25.1	18	0	14	4
36	25.4	17	1	14	4
37	25.6	18	0	17	1
39	26.5	15	3	12	6
Average...	18.8	16.0	2.0	14.2	3.8
		77.7%		57.7%	

operative scores for the two groups are 58.9 per cent[6] for the contralateral group and 57.7 per cent[6] for the homolateral group. From these results it is clear that there is a reduction in the ability tested following cerebral destruction, but no difference between the results of lesions in the contralateral and those in the homolateral hemispheres.

Conclusions for intelligent behavior

The results of unilateral cerebral destructions clearly indicate that for the function of intelligence in the rat as determined by Maier's reasoning test, there is no evidence for hemispheric cerebral dominance. The results are strikingly in favor of the theory that the hemispheres are equipotential for intelligent behavior in the rat.

DISCUSSION

The foregoing results show that in the rat, as in man, we have three psychological processes which are affected by cortical lesions, namely: (1) the motor function of handedness, (2) the motor response to a visual pattern, and (3) intelligent behavior. The motor function of handedness appears to be localized in a critical area of the anterior portion of the contralateral hemisphere. The visual perceptive process appears less localized, but has been ascribed to the posterior third of the cortex or to the striate area; while intelligent behavior is not localized but is considered a general function of the whole cerebrum.

In man, an exhibition of a dominant hand has been taken to indicate a dominant hemisphere, not only for the motor function of handedness, but for the visual perceptive function and for intelligence. One might assume similarly that since the rat exhibits a definite hand preference it also has a dominant hemisphere for the motor function of handedness, for the visual perceptive process, and for intelligence.

The results of this investigation, however, clearly indicate that in the rat the hemispheres are equipotential for the functions of pattern vision and intelligent behavior even though this animal does exhibit a definite hand preference, and reverses this preference after a cortical lesion in the critical area of the contralateral hemisphere. It follows, therefore, that the assumption of a dominant hemisphere for the visual function and for intelligent behavior, based on the exhibition of a preferred hand, is unfounded.

For those who propose the theory of hemispheric cerebral dominance in man, we are asking, not that they accept an analogy from rat to man, but that they advance some evidence for the theory other than speculation or interpretation from inadequate clinical data. This experiment indicates that the broad generalizations which have been made concerning the relationship of unilateral cerebral dominance to speech, reading, and writing, are not warranted from merely the determination of the handedness of the subject.

SUMMARY AND CONCLUSIONS

The purpose of this investigation was to furnish experimental evidence in the case of the rat for one or the other of the opposing assumptions: (1) hemispheric cerebral dominance; and (2) hemispheric equipotentiality.

A number of rats were (1) tested for hand preference; (2) trained to react positively to one of a pair of asymmetrical visual patterns (F against mirrored F); and (3) tested for intelligent behavior. The animals were divided into two groups. For those in one group a portion of the cortex was destroyed in the contralateral hemisphere (opposite to the preferred hand), while for those in the other group similar cortical lesions were made in the homolateral hemisphere (same side as the preferred hand). After allowing ten days for postoperative recovery, the animals were again tested for the three functions.

The results of the experiment warrant the following conclusions:

1. Cortical destruction within a critical area of the anterior portion of the contralateral hemisphere changes the hand preference of the rat. A lesion in any portion of the cortex posterior to the critical area of the contralateral hemisphere, or in any portion of the homolateral hemisphere, does not affect the rat's hand preference.

2. The cerebral hemisphere which would be considered "domi-

nant" for handedness (in the conventional use of the term) does not appear to be dominant for the visual function. The results showed that the retention of a motor response to a visual pattern suffered the same loss from a homolateral lesion as from a contralateral lesion. The hemispheres appear to be equipotential for this function. Furthermore there was no indication of a reversed response or "strephosymbolia" following contralateral lesions.

3. There appears to be no evidence for the theory of cerebral dominance as applied to intelligent behavior in the rat as determined by Maier's reasoning test. The results were strikingly in favor of the theory that the hemispheres are equipotential.

4. The results of this investigation are not directly applicable to man. However, the assumption that a preferential use of one hand is an indication of the dominance of one hemisphere for the visual function or for intelligent behavior was not confirmed by our data. On the contrary the results indicate hemispheric equipotentiality. Therefore until some direct experimental evidence is produced which shows the relationships of cortical functioning of handedness and the cortical functionings of other psychological processes which have been attributed to unilateral cerebral dominance, one is warranted in being very skeptical of the theory of hemispheric dominance and its various ramifications in man.

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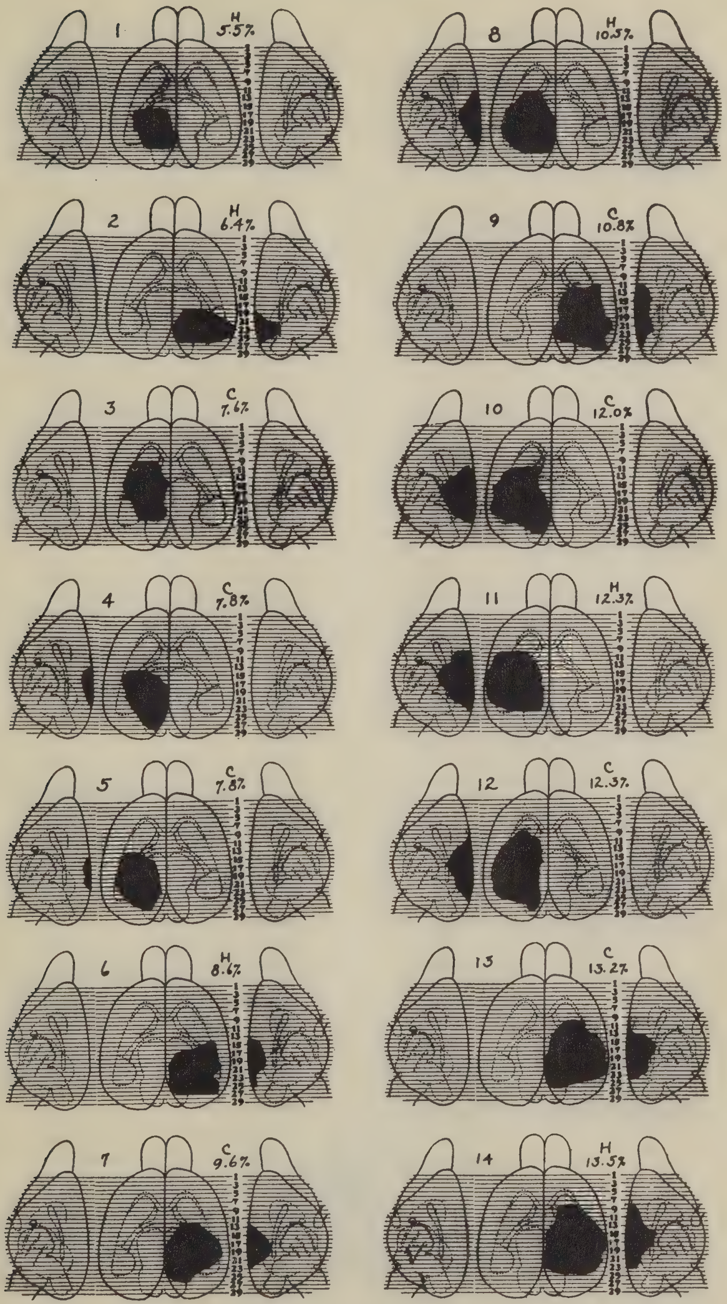
PLATES I–III

Plates I, II, and III are diagrammatical representations of the extent and locus of cortical lesions. The numbers of the rats and percentages of lesions correspond to the numbers and percentages in the tables. H refers to a homolateral lesion (a lesion on the same side as the preferred hand); and C refers to a contralateral lesion (a lesion on the opposite hemisphere to the preferred hand).


1
H
5.5%
8
H
10.5%
2
H
6.4%
9
C
10.8%
3
C
7.6%
10
C
12.0%
4
C
7.8%
11
H
12.3%
5
C
7.8%
12
C
12.3%
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C
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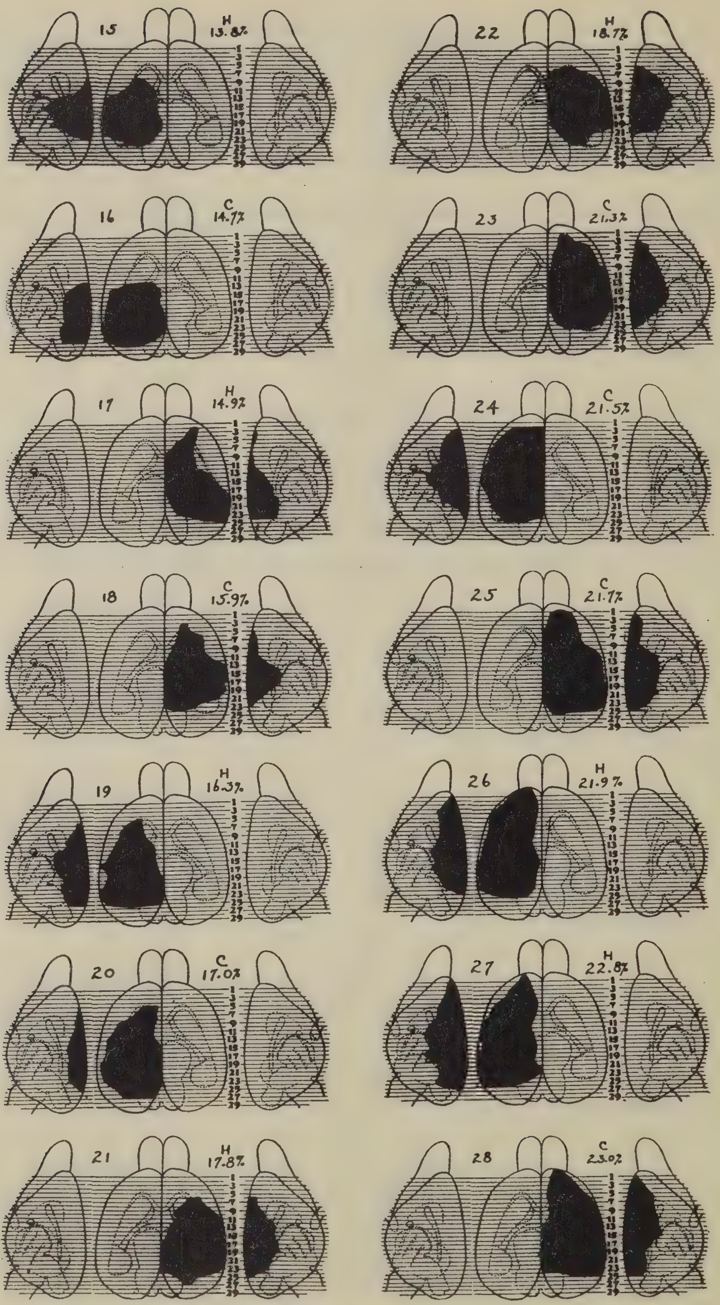
7
C
9.6%
14
H
13.5%

PLATE 1


15
H
13.8%
22
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18.7%
16
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14.7%
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C
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21
H
17.8%
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C
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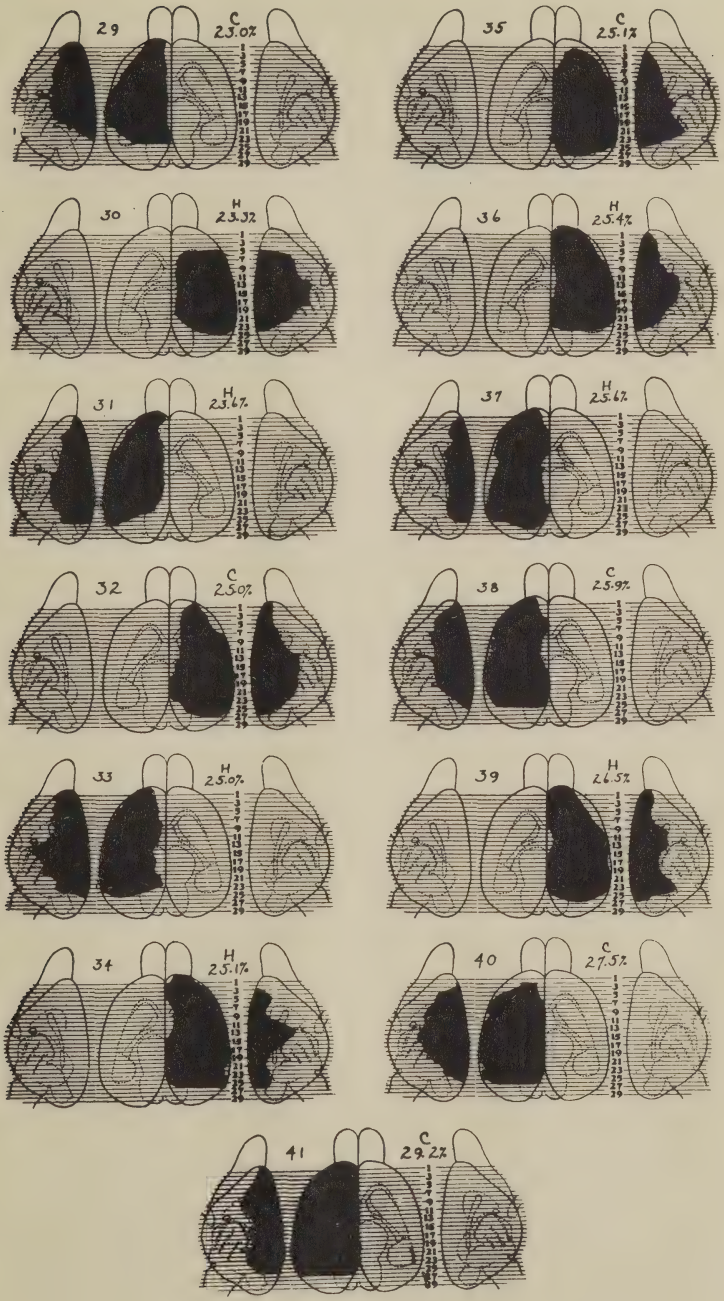
PLATE 2


29
C
23.0%
35
C
25.1%
30
H
23.3%
36
H
25.4%
31
H
23.6%
37
H
25.6%
32
C
25.0%
38
C
25.9%
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H
25.0%
39
H
26.5%
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H
25.1%
40
C
27.5%
41
C
29.2%
1
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5
7
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PLATE 3

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